THURSDAY, JUNE 20, 1878

TERTIARY FLORA OF NORTH AMERICA

United States Geological Survey of the Territories—Tertiary Flora. By L. Lesquereux. Vol. vii. (Washington, 1878.)

THE volume for the present year, of considerable thickness,[1] is entirely occupied by a most important work upon the Tertiary flora of North America. Although the geological and stratigraphical portions of the work are not so distinctly set out as they might have been, this by no means lessens the palæontological value of the work which is more especially in the author's province.

The country occupied by the Lignitic beds, described in the present volume, is a plain stretching for 600 miles between the Missouri and the Rocky Mountains. The ascending gradient is not more than ten feet per mile and the beds are horizontal; the whole Lignitic series is therefore exposed in sequence from east to west, commencing with the Cretaceous lignites, described in a previous volume (which occupy about one-third of the area), and ending with the newer Miocene or perhaps Pliocene beds.

The Tertiary Lignitic coal-fields of Western America alone, were estimated by Taylor in 1848 to occupy the enormous area of 250,000 square miles, or twice the area of Great Britain; but M. Lesquereux supposes that coal-beds of other ages must be included, and he points out (p. 7) that nothing positive was known of the great North American Lignitic until Dr. Hayden began his researches in 1854, and to the remarkable accuracy of whose work he pays a well-merited tribute.

Their area on the Upper Missouri is stated at not less than 100,000 square miles, and in the succeeding paragraph it is estimated at about 140,000 miles. This is their extent in that region of the United States alone, but their actual extent is much greater, since they stretch far across the border into British possessions; they have been traced southward along the base of the Laramie range to beyond the Arkansas River, and by outliers as far as Albuquerque, and again westward they are connected with the coal-fields of the Great Colorado Basin and the Laramie Plains.

Under the heading "Stratigraphy" we are told that there is no unconformability, physical, or other break in the sequence from the so-called Cretaceous lignites to the Tertiary Lignitic. It is then mentioned that there is a transition bed of coarse sandstone, irregularly deposited, whose thickness is not stated. There are next given detailed sections, some of Upper and some of Lower Lignite, none of them, however, presenting a thickness of more than 480 feet. It is strange that only a small paragraph (p. 12), extracted from Hayden's Report in 1874, to which no prominence is given, warns us that the Lower Lignites are of vast thickness—3,000 to 5,000 feet, but whether this thickness is all of Tertiary or the older lignites we are left to ascertain elsewhere, whilst we only incidentally gather that there is an Upper and a Lower Lignite. The faulty construction of the introduction arises from M. Lesquereux having too loosely strung together extracts from various works, which render it so disconnected as to be all but useless except to those who possess a previous knowledge of the subject. The thickness of, and as far as possible, the area occupied by, each division of the Lignite should have been as clearly set out as we find them in Hayden's Report for 1874, which is frequently referred to in the present work.

The third section is almost wholly occupied by verbatim extracts from the arguments of Professors Hayden, Meek, and Cope as to the age of the Lignitic. The conflicting nature of the evidence is apparent, the balance, apart from the botanical evidence, not here referred to, being decidedly in favour of Tertiary age. In the seventh chapter of the Annual Report, 1874, Peale had already given, in tabulated form, the views of authorities in regard to the age of each group of the Lignites which have been referred to Tertiary. In a much condensed form this might have been introduced into the present work with advantage. None of the Lignitic freshwater mollusca have been specifically identified with foreign forms; the vertical distribution of these being well-marked and limited, the evidence they would present should be of great value.

In this section (p. 24) a table is introduced incidentally, showing four groups of strata, which, omitting the lithology, the column of localities and all mention of fossils other than vegetable, reads thus:—

Names.	Fort Union group; Lignite group.	Wind River deposits.	White River group.	Loup River beds.
Sub-divisions.	"Great number of dicotyledonous leaves, stems, &c.; *Platanus*, *Populus*, &c.; with very large leaves of true Fan Palms."	"Petrified wood."	"Petrified wood."	[No vegetable remains mentioned.]
Thickness.	2,000 feet or more.	1,500 to 2,000 feet.	1,000 feet or more.	300 to 400 feet.
Foreign equivalents.	Eocene.	?	Miocene.	Pliocene.

From this table we should certainly conclude that the leaf remains were confined to the "Lignite group," and consequently that the botanical part of the work referred only to fossils of Eocene age. The work being on the "Lignitic" we are still further confirmed in this opinion, and dismiss the overlying "deposits," "group," and "beds" from our minds. With this impression we come to "Part II. *Description of the Tertiary Fossil Plants,*" and are in no way prepared for such a surprise as is reserved for us in Part III., 273 pages further on, on finding that the leaves are figured and described from beds of Eocene, Miocene, and Pliocene age indiscriminately.

In the second and third parts M. Lesquereux is evidently working at more congenial subjects, and deserves praise for the painstaking way in which he has described the material before him. The truism that all determinations based upon leaves alone are provisional and untrustworthy, has deterred English botanists from working at our Tertiary floras; and thus while we see magnificent works on this subject brought out in France, Germany,

[1] About 380 pages and 65 plates.

Austria, Italy, Switzerland, and America, no work of any importance has appeared in England. But though they are so, this in no way lessens the value of descriptions and figures for the purpose of comparing different floras, their distribution, the climatal conditions of each age, and inferring the relative age of isolated remains of land surfaces, volcanic outbursts, elevations, &c. It matters less whether a widely spread leaf-form is referred to oak or beech, than to ascertain that it is characteristic of a definite age and had a definite distribution. Fortunately there are palæo-botanists like M. Lesquereux who, having done their utmost to assign a leaf to its right genus, are content to wait for certain proof until the discovery of fruits and more perfect specimens.

Among the more interesting plants described are a *Lycopodium* and three species of *Selaginella*, all well preserved. The discovery of these is remarkable, as M. Lesquereux states that none of the Lycopodiaceæ had previously been known between Oolitic and recent times. The ferns are also of especial interest. Besides *Lygodium* and *Pteris* (which forcibly recall both Eocene and Miocene European forms), we have in *Gymnogramma Haydenii*, from the Lower Lignite, a form also common at Bournemouth (now described from more perfect fronds as an *Anemia*), and to the Aix-la-Chapelle flora as *Asplenium Forsteri*, Deb. and Ett., and to Sézanne as *Asplenium subcretaceum*, Saporta. Conifers and palms are numerous, but being fragmentary are less easy to compare with those of other localities. Still less so are other Monocotyledons, except a single Miocene *Smilax* leaf identified with a European species.

The Dicotyledons are very numerous, 212 species being described. Of these the leaves ascribed to *Myrica*, a large group mostly from the Upper or Miocene series, have an Eocene aspect. The forms assigned to *Betula, Alnus, Carpinus, Corylus*, and *Fagus*, are very similar to each other, and appear, from the figures, to have perhaps been too much subdivided. Any of these might be identified with leaves from Bournemouth, where the beds are undoubtedly Eocene. The leaf-forms from the Lower Lignite described as oaks have been determined with considerable hesitation, and are of most dissimilar character, as we see indicated by such specific names as *Q. platinia, Q. viburnifolia, Q. negundioides*. They have been classed as oaks on account of greater or less resemblance to species so described by European authors holding very diverse views. The single *Castanea* is indistinguishable from an Alum bay-leaf. In the simple forms of the Saliciniæ we have perhaps the nearest approach in character to European forms, and a large proportion of them will be found identical with leaves from Bournemouth. We must not, however, attach undue importance to the apparent similarity of simple ovate or lanceolate leaves, especially when comparisons are made from drawings only. The poplars form a large group, but, considering the variability of their leaves, species appear to have been unnecessarily multiplied, some being determined from fragments of leaves and others from single specimens, as *P. melanarioides, P. melanaria*, &c. *P. Zaddachi* is a familiar leaf in the English Bagshot and Woolwich beds, and in the Arctic beds, so-called Miocene; *P. Richardsoni* also appears identical with this form. Leaves, so similar to each other in outline and variation, that from the plates they can scarcely be distinguished, are described respectively as *Quercus platania, Populus lævigata*, and *Platanus Raynoldsii* and we long to know why they are so distinctly separated. The only leaf ascribed to *Ulmus* is from the Miocene stage, but appears identical with a Bournemouth form. Again *Quercus acrodon, Planera Ungeri* and *Fagus feroniæ* could all be matched with leaves from Bournemouth, and so resemble each other that it appears strained to have separated them under three genera and to form two new species from such material. On the other hand, one of the five figures (Fig. 5, pl. 28) ascribed to *Ficus lanceolata* is quite distinct from the rest and is certainly not that species, but a common British Eocene form. *Ficus multinervis*, Heer, is one of our most abundant Eocene plants, and, as suggested by Saporta, is more probably a *Laurus*. Leaves referred to *Ficus oblanceolata*, Lesq., analogous to another of our species, might reasonably have been separated as two species. As a group, the leaves called Ficus remind us of those met with in the Eocene. A remarkable feature in the flora is, that but a single form is referred to the Proteaceæ, and from its characters it seems unnecessary to refer even this to that group. Of the Lauriniæ, *Laurus* has an Eocene aspect, whilst the species of Cinnamonum appear correctly identified with Miocene forms. The nine species of *Viburnum*, which would perhaps have been better reduced to two, are analogous to a Bournemouth leaf, but still more so to the *Viburnum* of Sézanne. A leaf referred to the Australian genus, *Callicoma*, is of simple lanceolate form, with serrated edge, whilst two other forms referred to *Ericaceæ* are very indistinctly characterised. Many of those named *Sapindus* and *Diospyros*, as well as *Zizyphus* and *Rhamnus*, are of essentially Eocene facies and very similar to Bournemouth forms. The leaves placed together as *Ilex dissimilis* are so unlike that it seems doubtful whether they could have belonged to the same species.

The apparent defects which are here pointed out, may be partly due to imperfect figures, and reference to the specimens themselves might uphold the correctness of M. Lesquereux's separate determinations, since the specific identity or otherwise, of leaf forms, is often, after all, very much a matter of individual opinion.

The third part of the work contains a tabulated list of all the plants with the relative position of the beds from which they are derived, and also their possible relationship to those of the Eocene and Miocene strata of Europe. This is followed by a careful digest of the matter contained in the work, from which the following important facts are to be gathered. The lower group contains 200 out of the 325 species described, and is so isolated that but sixteen of the forms pass into the higher tertiaries, and these include none of the essential types, as the palms, magnolias, *Grewiopsis, Viburnum, Rhamnus*, &c. The second group has thirty-four species, twenty peculiar to it, and is, on the whole, correlated rather with the overlying Miocene than with the Lower Lignitic. The third group is unhesitatingly pronounced to be Middle Miocene, on account of the relation of its plants to the so-called Miocenes of Alaska, Greenland, Spitzbergen, and Europe; and it is further said that no Eocene type is present in the group. The fourth group is also

Miocene, and indicates a temperate climate such as that now prevailing in the middle zone of the United States, as from Ohio to North Alabama. The larger number of its species are identified with, or analogous to, those of Greenland, Spitzbergen, and Alaska, whilst a few are related to Pliocene plants, and three species are still living. Mr. Lesquereux devotes the concluding pages of his work to proving the Tertiary age of the Lignitic series. If, as he states, he has correctly ascertained that, in the first or lowest group, 120 species represent Tertiary, and only six can be considered at all as Cretaceous forms, he has made good his case, and all European palæontologists will agree with his views as to the age of the Lignitic.

The study of a very large series of British Eocene plants in my own collection, from well-defined horizons, has enabled me to draw somewhat different conclusions from those of Lesquereux and Heer. Unfortunately no great and undoubtedly Eocene flora has ever been described or published, and I therefore use the Bournemouth as a typical Eocene flora. The flora of Oeningen, made so familiar to us by Heer, is a typical Miocene flora, and although most unlike a true Eocene flora, contains many plants common to other isolated fragments of strata which contain mixed floras, that is, floras with percentages of Eocene as well as Miocene plants. There being no typical series from the Eocene available as a standard of comparison, the plants common to the Miocene have alone been taken to determine the age of these beds, and the unknown Eocene forms have thus been enrolled as Miocene, and in their turn used to identify other still more distinctly Eocene beds as Miocene; much in the same way as the Barton beds were formerly identified, from their possessing a few species in common, as London clay, and the species peculiar to the Barton horizon subsequently made use of to identify Calcaire-Grossier and Bracklesham beds in their turn with the London clay. The errors which have thus possibly been committed even by Heer, who has been led to class all the many floras he has so ably described, either as Cretaceous or Miocene, were therefore unavoidable, and scarcely reflect upon his judgment.

The Lower Lignite is, in my opinion, undoubtedly Eocene, and probably contemporaneous with our London Clay or Lower Bagshot. The sudden incoming of palms and European plants of tropical kinds, and of mammals, and the displacement of the indigenous and temperate flora and of the lingering Dinosaurians, is evidence clear and unmistakable, that the continents became united at this period. Simultaneously with this sudden increase of temperature in America we find a corresponding increase in Europe, as seen on comparing together the faunas of the Thanet Sands and London Clay. The increase was in all probability due to the final rise of a land barrier completely shutting out all the cold northern currents, which at the present day set towards the equator, and so materially modify the ocean temperature. I think we are thus able to fix the comparative age of the Lower Lignitic, which, being upwards of 2,000 feet thick, probably required a great part of our Middle and Upper Eocene period for its deposition. I regard the second group as of our Upper Eocene age, and the third and fourth groups as Miocene.

Now comparing Dicotyledonous leaves of the Lignitic flora with those of Arctic regions, we find several, even of those from the Lower Lignite, common to both. The greater part of these are included in the following list:—

Viburnum Whymperi, Heer.
Fraxinus denticulata, Heer.
Diospyros brachysepala, Al. Br.
Andromeda Grayana, H.
Cissus tricuspidata, H.
Vitis Olriki, H.
Ficus tiliæfolia, Al. Br.

We find common to the Lower Lignitic and the Miocene of Switzerland—

Quercus neriifolia, Al. Br.
„ *chlorophylla*, Ung.
„ *Godeti*, H.
Salix angusta, Al. Br.
Populus melanaria, H.
„ *mutabilis*, H.
Ficus tiliæfolia, Al. Br.
Cinnamonum Scheuchzeri, H.
„ *polymorphum*, Al. Br.
Daphnogene anglica, H.
Diospyros brachysepala, Al. Br.
Cornus Studeri, H.
Berchemia multinervis, Al. Br.
Rhamnus alaternoides, H.
„ *rectinervis*, H.
„ *Rossmässleri*, Ung.

or far more than sufficient to have identified the formation with Miocene had its true position not been otherwise ascertainable. None of these are, however, very distinctive leaves, and, with very few exceptions, they might, had the English Eocene Flora been published, have been referred to it with greater approximation to certainty. The exceptions are of little value, *C. Scheuchzeri* being identified on half a leaf, while the references to *P. mutabilis* and *C. polymorphum* are extremely doubtful. The truth is that so many of the ovate and lanceolate leaves of the Miocene and Eocene resemble each other that it would be easy to compile a sufficient list to refer, with plausibility, any given flora to either age, according to the author's fancy.

I have not yet had leisure to enter more minutely into the question, but it appears to me that the fact of a proportion of the Lower Lignitic leaves, which are of undoubted Eocene age, being also found in the Arctic floras, and the untrustworthy nature of the evidence on which these have been referred to Miocene, still leaves the question of their true age, on palæo-botanica evidence, unsettled. We know that in Eocene times these regions were land, and that floras existed upon them, and passed from one continent to the other, whilst in Miocene times, from the decrease in temperature, we infer that the submergence of this bridge had commenced. Further, the high temperature in the Eocene time would have permitted a temperate flora to grow in these latitudes, and the Miocene temperature would not. Lastly, it is difficult to conceive that the same quality o flora could have grown contemporaneously in latitudes so widely different as the United States and Greenland or Spitzbergen, but there is no difficulty in realising that a decreasing temperature, such as prevailed in the Miocene would have gradually driven the northern forms southward, and thus the very similarity of the Miocene flora of America to that of the Arctic circle renders it unlikely that they were of the same age.

J. S. GARDNER

FOURIER'S "ANALYTICAL THEORY OF HEAT"

The Analytical Theory of Heat. J. Fourier. Translated by A. Freeman. (University Press, Cambridge, 1878.)

THERE cannot be two opinions as to the value and importance of the *Théorie de la Chaleur*. It has been called "an exquisite mathematical poem," not once but many times, independently, by mathematicians of different schools. Many of the very greatest of modern mathematicians regard it, justly, as the key which first opened to them the treasure-house of mathematical physics.

It is still *the* text-book of Heat Conduction, and there seems little present prospect of its being superseded, though it is already more than half a century old. It contains the first satisfactory definition of Conductivity, the first statement of the *dimensions* of various physical quantities, and the invaluable expression for periodic quantities in terms of harmonics. Many important problems of heat conduction are completely solved, and the results are given so as to be immediately applicable in practice, as for instance to the cooling of spheres (including the secular cooling of the earth) the propagation of periodic changes of temperature into the crust of the earth, &c.

But the heat equations are of the same form as those in certain other branches of physics. Here they are solved once for all, and form a store from which all may freely help themselves. Thus, a very minute fragment of the work sufficed, by its application to electric currents, to render the name of Ohm famous. More important portions have been applied to Diffusion, to Signalling through Submarine Cables, and to various other important questions.

With all its transcendent excellences this great work had two faults at first, and of late it had acquired a third.

(1.) It was a little prolix. Like Ampère's great work, and some others of that wonderfully fertile period, it was made up as a sort of patchwork of memoirs sent to the French Institute. Each memoir was, as it were, complete in itself: and the putting together into one work, without judicious paring down, necessarily involved a good deal of repetition.

(2.) It was so full of printers' blunders and mere slips of the pen that it must have been very carelessly revised.

(3.) It had become very scarce, and consequently expensive.

The Syndics of the Pitt Press deserve great credit for reproducing the book:—and the printers have done their share of the work well. Still, the result can hardly be called satisfactory. For this there are many reasons.

(1.) We think it was a great mistake to translate the book into English. The poetry, except so far as it was in the formulæ, is gone; and the prolixity, which was tolerable in the original, is painful in the translation. The text should have been considerably compressed in translation, or else simply reproduced in French. Every one who has any right to read Fourier reads French, or at least ought to be able to do so. Again, though *Conducibilité* and *Conductibilité* are good French, Conductibility (being altogether erroneous) has hitherto been confined to the lowest class of English books. CONDUCIBILITY, which Mr. Freeman most commonly employs, is not an English word at all;[1] and, even if it were, could not possibly mean Conducting power, or Conductivity.

(2.) We have compared at least one whole chapter with our own annotated copy of the original. Roughly speaking, only about 50 per cent. of the misprints in the original have been corrected. The others, some very misleading, are reproduced. The worst of those we have noticed are at

pp. 124 [Eq^{n}. (*a*)], 134, 189, 226.

In p. 181 an erroneous reference is reproduced, and in order to make it fit the text the reference mark is shifted from the general equation (really referred to) to a mere particular example.

(3.) The translator has added a few notes, some by the late Leslie Ellis. But they are very fragmentary. Surely more than a single sentence might have been devoted to the experimental results of Forbes [and Ångström]; Stokes and Duhamel ought to have been mentioned with reference to conduction in non-isotropic solids—and Thomson's proof that Fourier's solution of the problem of the cooling sphere is *complete* deserves much more than the mere casual mention it has received.

OUR BOOK SHELF

Anthropology. By Dr. Paul Topinard, with a Preface by Prof. Paul Broca, translated by Dr. R. Bartley. (London: Chapman and Hall, 1878.)

THIS volume forms another of the Library of Contemporary Science, and it purports to elucidate a science which is well described by Paul Broca as being one of vast dimensions and one in process of rapid development, as well as one which has hitherto not received sufficient attention. The masters of the science engaged in original research naturally shrink from the labour of writing a handbook of a popular character: and it fell to Dr. Topinard's lot to make the attempt—in which attempt he seems pretty fairly to have succeeded. This work falls into three sections: the first treats of the physical characters of man, and of his place in nature. The chief human anatomical peculiarities are briefly alluded to, with a somewhat needless—to our mind—reiteration of the assertion that the organisation of anthropoids is a counterpart of that of man, and differs widely from that of the other Simian groups. The second section treats of the races of mankind; and here we have a great many important and interesting facts marshalled in fair order before us. A few more woodcuts would have been an improvement to this portion. In the concluding section the origin of man is discussed; and the author passes in array the monogenestic theory of Quatrefages, the polygenestic theory of L. Agassiz, the transformation theory of Lamarck, and the natural selection theory of Darwin, and works out in detail the application of each to man and his genealogy. The translation, which is generally good, might, however, in places be improved, and it is sometimes a little confused.

[1] On reference to Richardson we find one instance of the use of the word, by (Bishop ?) Wilkins. We freely give Mr. Freeman any benefit which he can extract from the following passage:—

"Duties deriving their obligation from their conducibility to the promoting of ends."

It may interest readers of NATURE to be told that, in looking for the word in the Supplement to the *Imperial Dictionary*, we found the following extraordinary statement (illustrated by a diagram) about Conjugate Foci:—"when rays, falling upon a lens, are so refracted as to converge and meet in a point, either nearer the lens than the principal focus, or farther from it, the point in which they meet, and the principal focus, are called, with respect to each other, *Conjugate Foci*."

The Tailed Amphibians, Including the Cæcilians. A Thesis presented to the Faculty of Michigan University by W. H. Smith. (Detroit, Michigan, 1877.)

THE title of this little volume tells its own story. It is a detailed catalogue of all the species of tailed amphibia known. In addition to using the works of all the best writers on this group, Mr. Smith has availed himself of the specimens in his University Museum, and from these has drawn up many of the descriptions and characters. A number of artificial keys are given to the genera and species; the synonymic lists appear to have been worked out with care, and to have been brought down to date. A list of authors on the subject of the work is appended, and here and there, after the diagnoses of the species, will be found details of their habits, geographical distribution, and development.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

AGREEING in the main with the views put forward by Mr Archibald in his letter in NATURE (vol. xvii. p. 505), I beg leave to refer briefly to one or two points in which I differ from him, and I hope that you will be able to find space for this note, because Mr. Archibald has done me the honour of mentioning my name so frequently in his letter, that I might reasonably be supposed to entertain opinions identical with his own on all points regarding the question under discussion.

In the first place I would point out that the atmospheric current which brings the winter rains of Northern India, whilst it has nothing to do with the summer monsoon, does not descend in the Punjab, as Mr. Archibald says, and then proceed eastwards to the North-West Provinces and Behar, and sometimes even as far as Calcutta, but blows in just the opposite direction, appearing as a south-east wind over the Gangetic plain and the Eastern Punjab. The place of its descent in the winter months is farther south, in latitude 22° or 23° N., and thence it flows northwards in almost the same manner as the summer monsoon.

In the next place, I think the hypothesis of the approximately inverse variation of the winter rain, as compared with sun-spots, does not necessarily postulate a corresponding inverse variation of solar radiation. Such a relation I consider to be highly probable, but the somewhat meagre data I was able in a former communication (vol. xvi. p. 505–6) to adduce in favour of it were only intended to prove that the question of "solar activity" was yet an open one, and that it did not follow that solar radiation was most intense at times of maximum sun-spots, because many meteorologists, reasoning from magnetic and other analogies, assumed it to be so. The direct solution of the question must be accomplished by actinometric observations, as Mr. Blanford proposes, and, while it remains unsettled, it will probably be best to try and correlate the variations of rainfall with those of some other meteorological element upon which rainfall depends. I have recently been occupied with an analysis of the rainfall observations of twenty stations in Northern India, embracing between them 11° of latitude and 24° of longitude, and extending over periods of from fifteen to forty-nine years, and I find a remarkable coincidence between the variations of the winter rainfall and those of the temperature of the tropics as given by Köppen in his exhaustive paper in the *Zeitschrift der oesterreichischen Gesellschaft für Meteorologie*, vol. viii., Nos. 16 and 17. When the rainfall deviations of the different stations are thrown into the form of percentage variations from the local mean and are then combined and the results "bloxamed," we get a series of numbers which gives a curve from 1834 to 1877 resembling Köppen's curve very closely, when the latter is extended up to 1877. The two curves not only resemble each other in all their more important fluctuations, but their epochs of maximum and minimum approximately coincide. These are:—

Max.	Tropical Temperature	1842·7, 1854·7, 1865·1, 1876·3(?)
	Winter Rain	1842·7, 1855·0, 1865·5, 1876·9(?)
Min.	Tropical Temperature	1836·9, 1847·7, 1858·4, 1874·8
	Winter Rain	1837·8, 1848·1, 1860·6, 1874·7

It would therefore appear to be highly probable that the periodic variation of the winter rainfall of Northern India is caused by a corresponding variation in the temperature of the tropics, which determines, within certain limits, the quantity of vapour added to the air and the direction and velocity of the atmospheric currents. It appears, also, from the table, that the maximum of winter rainfall is attained nearly a year *before* the minimum epoch of sun-spots, as given by Wolf. I have found that this is also the case with the winter rainfall of London, and Mr. Draper has shown (NATURE, vol. xvii. p. 16) that the same relation holds good at New York.

The co-existence of severe droughts in Hindustan with devastating floods in Burmah and Assam, is a very strong argument against the theory of Dr. Meldrum that the rainfall of the whole globe varies directly with the sun-spots; but it would naturally follow from the view advocated by Mr. Archibald, because, in very hot years, which are approximately those of minimum sun-spot, the general tendency to a cyclonic circulation of the atmosphere round the Asiatic continent in the summer months would be so intensified as partially to obliterate the smaller cyclonic indraught towards Central India, which brings up a moist current from the Bay of Bengal to the Himalaya and the plains of Northern India. S. A. HILL

Allahabad, May 18

A Twenty Years' Error in the Geography of Australia

IN almost every detailed map of Australia, including some of the latest, we find, at the head of the Alligator River, in about S. lat. 13½°, and E. long. 133°, some such note as this:—"Steep walls, 3,800 ft." This is copied from the map illustrating "Leichardt's Journal," published in London in 1847. This map was (as stated in the preface) drawn by S. A. Perry, Esq., Deputy Surveyor-General of New South Wales, from materials furnished by Leichardt, and was engraved in London by Arrowsmith. As Leichardt only returned from his first expedition at the end of 1845 or beginning of 1846 he could have had no opportunity of correcting or revising this map. Mr. James Wilson, the geologist to the North Australian Expedition under Mr. A. C. Gregory, having passed over much of the same country, and finding the plateau nowhere more than 1,600 feet above the sea, came to the conclusion that Leichardt's supposed statement was an engraver's or printer's error which had escaped correction, and gave his reasons for this view in the *Proceedings* of the Royal Geographical Society, vol. i. p. 230, and subsequently in the same society's *Journal*, vol. xxviii. p. 137 (1858). Notwithstanding the extreme improbability—almost amounting to absurdity—of there being precipices of the enormous height of 3,800 feet, in a country where there were no important mountains, and where Gregory, who had passed within eighty miles, and M'Douall Stuart, who had passed within forty miles of the place, found nothing but a moderately-elevated plateau, with ravines never exceeding 600 feet deep, no notice appears to have been taken of Mr. Wilson's correction, but the "3,800 ft." has been copied again and again in works of reputation and authority. We find it even in the new edition of the "Encyclopædia Britannica," art. "Australia," given as an established fact in the following words:—"On the north side of the continent, except around the Gulf of Carpentaria, the edge of the sandstone table-land has a great elevation; it is cut by the Alligator River into gorges 3,800 ft. deep."

The curious thing is, however, that this marvellous phenomenon, which, if it existed, would be unapproached in Australia and equalled nowhere but among the mountains of the great continents, is not even alluded to in the published journal of the traveller who is supposed to have discovered it! On Leichardt's *map* the "steep walls" are noted between the stations of November 10 and 11, but in his "Journal" we find no reference to anything remarkable till November 17, when he comes to the head of a magnificent valley, into which he was obliged to descend, and which caused him much delay and circuitous explorations on account of its steep rocky walls estimated by him to be "1,800 feet high." It is pretty clear, then, that the

"3,800 feet" is a map error, and that even the 1,800 feet is merely an estimate, and probably an over estimate; for we must take into consideration the evidence of other explorers in the same region, and the appalling effects of coming, in a nearly level plateau, to the brink of such a precipitous rocky barrier.

I am making a similar correction to the above by means of a note in a work I am now engaged upon (on Australian Geography), but as the error has obtained such wide circulation and seems so hard to kill, it becomes advisable to call attention to it as soon as possible, and in a way that will be likely to attract attention. ALFRED R. WALLACE

Opening of Museums on Sundays

YOUR last number contains a letter from my friend Prof. Corfield, which I confess to having read with some little astonishment. He expatiates, and with justice, on the merits of the town of Maidstone, whose citizens do not scorn the grace which "palæontological, conchological, and other collections" must add to life spent in a country "well worth visiting," and who appropriately find their last resting-place in a cemetery "which is one of the most beautiful in the country." I would not demur a moment to such a fascinating picture, were it not that Prof. Corfield, led away by a pardonable enthusiasm, expresses his belief "that this is the first and only scientific museum that has yet been opened on Sunday in the United Kingdom." Surely the Chairman of the Committee of the Sunday Society need not go to Maidstone for the first victory in the very just cause which he upholds, seeing that for the last quarter of a century the three buildings which contain the Botanical Museum of the Royal Gardens, Kew, have been open to the public from two till dusk every Sunday throughout the year.

Royal Gardens, Kew W. T. THISELTON DYER

Socialism in South Africa

I NOTICED this morning that along the bottom of the front wall of my house, on the verandah, there lay a quantity of reddish-brown powder; there was enough to fill a coffee-cup. On looking closer I saw that it was made up of small and larger fragments which glistened, and on inspecting some in my hand they turned out to be the heads, legs, trunks, &c., of countless ants. A number of these animals were still on the wall above, and my attention being now arrested, I watched them, and saw that they were contributing to the carnage beneath. This species of ant is a small, comparatively harmless one, the chief sin of which is that it makes its way to every species of food and swarms on it. As is usual with ants, the general body of insects is accompanied by larger individuals, which are provided with heads and jaws quite disproportionate to their bodies, and with these jaws they do all the cutting up. Among the ants on the wall there was a large sprinkling of these "soldier ants," and the whole community seemed to be bent on destroying them. The proportion of heavy-jawed to ordinary ants was about one to ten. I saw a group of little ones fastening on to a big one, which made desperate efforts to release itself. At first the big one bit several little ones in two, and the parts dropped down from the wall; but after a while the little ones severed all the legs of the big one, and finally got on his back and cut him in two. The group then dropped down to swell the mass below. Similar scenes were enacted elsewhere on the wall. The commencement of one combat was as follows:—A big ant walked along till it met another big one, and the two shook antennæ. Just then a little one seized hold of a hind leg of one of these big ones. Neither took any notice, but continued a rapid conversation. Suddenly other small ones came up, when the big one whose leg was grabbed turned furiously on the little one and seized him by the middle. This could not be done until the big one had doubled himself up; as soon as he had hold of his small antagonist he lifted him in the air and snipped him in two. Meanwhile all the big one's legs had been seized by little ones, and the party seemed to turn over and over, little bits tumbling down, now a leg, now half an ant, till the big one was vanquished.

The ant is most assuredly subject to passions. The way in which the big ant turned on the little one was singularly indicative of rage. The determined manner in which he laid hold of the little one was quite human. If I had had a magnifying glass, the scene would have been really exciting.

Maritzburg, Natal, May 12 F. E. COLENSO

The Telephone Relay or Repeater

THE writers have been at work since the announcement of the invention of the Bell articulating telephone in endeavouring to devise means by which the telephone might be relayed. Quite a number of devices have been tried, but, from the exceedingly feeble amount of the movements of the diaphragm of the receiving telephone, they have heretofore been unsuccessful in obtaining any practical results.

The discovery by Prof. Hughes of the inexpressibly delicate microphone has given us the means by which we have *finally* at last solved this very important problem. We apply the microphone as a telephone relay or repeater by attaching it directly to the diaphragm of the receiving telephone. The microphone so attached is a miniature one consisting essentially of three pieces of carbon, arranged as described by Prof. Hughes. The two parallel pieces are cemented directly to the telephone diaphragm, and the third piece placed in cavities near their ends. The microphone forms, of course, part of the new circuit in which it is desired to repeat the telephonic message. By the movements of the telephone diaphragm the microphone produces such variations in the electrical current traversing its circuit as to cause the original message to be repeated to any instruments placed therein.

We have tried our telephone relay or repeater on several telephone lines, and find it to work satisfactorily. By attaching a number of miniature microphones to the receiving diaphragm and suitably connecting the battery, increased delicacy will undoubtedly be obtained. EDWIN J. HOUSTON

Central High School, Philadelphia, ELIHU THOMSON

U.S., June 7

New Form of Microphone Receiving-Instrument

HAVING been experimenting with the microphone, and studying the effect of the passage of the current on a galvanometer, it occurred to me that if the needles were *fixed*, strains would be produced in it by the action of the current. To test this, I passed a few yards of copper wire (about No. 30) on a small bar magnet *lengthwise*, and found, on placing it to the ear, that sounds were heard on interrupting the current; these sounds were much intensified by placing the magnet inside the lid of a pasteboard box.

Having a six-inch horse-shoe magnet beside me, I passed along *one of its limbs* from two to three yards of the same wire, and on placing the lid of a tin box on the *flat sides* of the ends of the magnet, an excellent receiving-instrument was obtained. With this tuning-fork, sounds, singing, whistling, speaking, and violin music were heard distinctly. A single Leclanché coil was used, the transmitter consisting of two small pieces of carbon pencil touching slightly, and connected with an open pasteboard box. W. J. MILLAR

Glasgow, June 17

A Waterspout

AMONG the meteoric phenomena of which we have heard recently, not the least interesting occurred on Thursday the 14th near the Kelston Round Hill, about three miles to the west of Bath. Shortly after five o'clock in the evening the inhabitants of the village of Weston, which lies between Kelston Hill and Bath, were startled by a volume of water advancing like a tidal wave along the Kelston Road; in a minute the water was upon them, flooding the houses and laying the main street four feet deep under water; with such force did it come that a stone weighing five hundred-weight was carried several yards, while smaller ones were taken a much greater distance.

It was not known in the village from where the water had come, but it so happened that about five o'clock I was proceeding to Weston Station by the Midland Railway from Bristol to Bath, and when in sight of the Round Hill I was struck by the blackness and lowness of the clouds in its vicinity. Suddenly there was a flash of lightning, and immediately after the Hill was enveloped in what appeared to be a storm of rain of unusual density.

On arriving home I was not altogether surprised to find the commotion in the village, and I at once attributed the source of the water to the cloud which I had seen; I therefore made my way in the direction of Kelston Hill.

On arriving under the brow of the Hill it was very clear that something more than an ordinary storm had occurred. Near the

end of a lane (Northbrook) leading to some fields, the hedge on the right for some yards was lying in the road, but the field beyond at this point presented only the appearance of an ordinary storm, while the lane itself was like the bed of a river. To the left was a field of standing grass; for about twelve feet from the hedge the grass remained intact, then for about the same distance it was as though it had been mown down. This torrent, for such it might have been compared to, came to almost a sudden termination a little above the end of the lane, but it extended down the Hill till it was joined by two others, one of which had carried a hedge away bodily.

The increased volume of water then poured down over some gardens, uprooting trees and vegetables; in less than ten minutes the hedges were lost sight of, and the water rose to a height of eight feet. This was occasioned by a block caused by an arch, which carried off the water from a small stream, not being large enough to take the increased volume. Finally it burst over, scooping the ground out in front of some cottages several feet deep and flowed on as a river some yards wide, again destroying gardens in which were valuable stocks of vegetables.

Near this point the volume of water was again increased; in all five distinct water-courses could be made out, all of which had done considerable damage to grass, cornfields, and gardens. Finally, all united in one body and poured into the village of Weston, levelling three walls as it came, and thence passed into the river Avon.

I gather from spectators at Kelston Hill that it began to be cloudy at half-past four in the afternoon; at five there was a rattling clap of thunder, followed by a downpour of rain—in "bucket-fulls," as one expressed it; but all seemed to agree that the greater portion of the water fell under the brow of the hill, where it came down in several columns. There were no houses close to the spot; had there been they must have been washed away.

The atmosphere had been perfectly still all day, but very sultry. Heavy rain fell in the neighbourhood, and the storm to which I have referred specially was accompanied with hail, which in a few minutes covered the ground some inches deep.

What I have described is no doubt what is popularly termed a waterspout.

The damage done was at first estimated at 2,000*l*., but it is now feared that this amount will not cover it.

Weston, near Bath, June 17 E. WETHERED

Fortunate "Escape"

AN evening paper of to-day's date has the following:—

"HOUSE STRUCK BY LIGHTNING.

"During the thunderstorm yesterday, at about 2.30 P.M., a large stack of chimneys at the residence of Mr. Robert Avis, at Putney, was struck by lightning, which split the chimney-shaft down the whole height, the electric current passing down the chimney and into a sitting-room on the ground floor. *The door of the room was fortunately open, and the current escaped without causing injury to the family*, who were in the room at the time of the shock."

The italics are those of one ELECTRIFIED

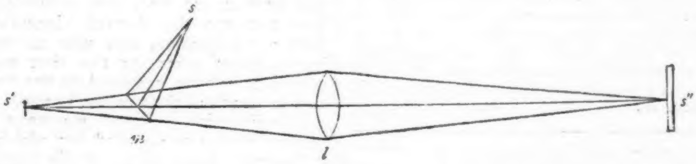
June 17

Velocity of Light

WILL you have the kindness to publish the following as a preliminary announcement:—

The following method of measuring the velocity of light dispenses with Foucault's concave reflector, and permits the use of *any* distance.

In the figure, s is a division of a scale ruled on glass; m, a revolving mirror, l, an achromatic lens; s'', a fixed plane mirror at any distance from l.



The point s is so situated that its image s' reflected in the mirror m is in one of the foci of the lens l, while the image of s' coincides at s'' with the mirror, the latter being placed at the conjugate focus. With this arrangement, when m turns slowly, the light from s'' is reflected back through the lens, so that an image is formed which coincides with s. When, however, the mirror rotates rapidly, the position of m will have changed while the light travels from m to s'', and back again, so that the image is displaced from s in the direction of rotation of the mirror.

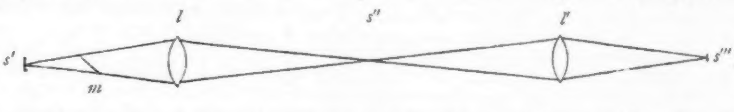
Let V be the velocity of light; D, twice the distance $m\,s''$; n, the number of turns per second, and r the distance $m\,s$; then, calling δ the deflection, V is found from the formula—

$$V = \frac{4\pi r n D}{\delta}.$$

In a preliminary experiment the deflection amounted to five millimetres when the mirror revolved 128 times per second.

The following is another plan which would probably give more light than the above.

s is as before the image of the scale reflected in the mirror m;



its image would be formed at s'' by the lens l, and the image of s'' would be formed at s''', where the plane mirror is placed. In this case also, the rays are reflected back, so that the scale and its image coincide notwithstanding the (slow) rotation of m. ALBERT A. MICHELSON

U.S. Naval Academy, Annapolis, Maryland

University College

THE fiftieth anniversary of the opening of University College falls within this year. It is intended to celebrate the occasion by a gathering of members of the corporation, present and past, professors and masters, old students of the college and school, and other friends and benefactors of the institution, to be held within the precincts of the college, on Tuesday, July 9, at 1 o'clock P.M. The Right Hon. Earl Granville, K.G., Chancellor of the University of London, has kindly accepted the invitation of the President, Council, and Senate to attend and lay the first stone of a further extension of the college buildings and preside at the luncheon; and the presence is expected of many other persons of distinction interested in the welfare of the college and in the promotion of University education.

The space at the disposal of the college, even since the school has been entirely withdrawn to the south wing, is far from adequate to the rapidly increasing requirements of modern education. The Fine Art Department has been obliged to refuse pupils. The Council has, moreover, after prolonged experience

of the satisfactory working of the Ladies' Educational Association, recently decided to open the Faculties of Arts and Law and of Science to women.

Again, there is a very general demand for increased facilities of instruction in engineering and other branches of applied science, which can nowhere be so efficiently met as in connection with a flourishing scientific school like that of University College. Lastly, the numbers of the school have for some years been steadily increasing, and it is not unreasonable to hope that it may soon outgrow its present space. On all these grounds an urgent necessity is now imposed upon the college to undertake a considerable enlargement of its buildings.

Application for tickets should be made to the Jubilee Celebration Committee as early as possible. TALFOURD ELY,
University College, London, June 18 Secretary

Examination of Small Organisms in Water

IN order to examine the minute organisms that inhabit water, such as rotifers, vorticellæ, and kindred microzoons, the arrangement I proposed some years ago in the *Quart. Journ. of Micros. Sci.* will, I believe, be found most convenient. This is to inclose the objective in a brass or other metal tube having its lower end closed by a piece of thin microscopic glass coming close up to but not touching the object-glass. With this protection we can plunge the end of the microscope into a small tank, filled with water, containing the small living organisms, and examine them at our leisure for days or even weeks. The thin glass plate immersed in the water gives us a perfectly steady, flat water-surface, which is not disturbed by any agitation of the surface-water of the tank. Objectives of an inch, half an inch, a quarter of an inch, and even an eighth of an inch focus, may be thus used under water, and all the trouble of catching and ensnaring the small animals is thus avoided. This invention I first employed for the examination of morbid secretions, such as urine. I have since employed it for watching the operations of minute creatures that inhabit water, which may thus be seen in their natural habitat and under normal conditions, which is not the case when they are seen in the usual way, between the two layers of glass on an ordinary microscopic slide. Any optician can make such a tube to screw over the objective of any microscope, and, though it can readily be removed and applied, its presence does not interfere with the use of the microscope in air.

53, Montagu Square, W. R. E. DUDGEON

THE LATE MR. HEWITSON

THE memory of the warm-hearted gentleman above-named deserves a passing notice in these columns, for the effect of his labours on at least one department of natural history has been great. William Chapman Hewitson, who died at Oatlands, near Walton-on-Thames, on May 28 last, aged seventy-two years, was by birth a Northumbrian, and, after the somewhat rough education of a Westmoreland school, took up the calling of a surveyor. His passion for natural history was exhibited in very early life, and, after some years' practice of his profession, the fortunate inheritance of a competence, and something more, from an uncle saved him the necessity of pursuing a distasteful vocation, and enabled him to indulge his fancy practically without stint. In 1831, while still engaged in his professional duties, he projected his "British Oology," the first part of which appeared in April, 1831, and the last in 1838. As he himself subsequently wrote:—

"The book was itself as migratory as the birds, the eggs of which are depicted in its pages; many of the plates were drawn at night after a long day of railway surveying in the fields, and the letter-press was printed wherever the author happened to be stationed at the time. There were few collectors to aid him in those days, and it is with a grateful feeling he remembers now the helping hand which was then held out to him by his friend Mr. Yarrell."

Yet the work was a great success. Such beautiful figures of eggs—all drawn on stone by the author—had never before been seen, for his touch was as delicate as his eye was correct, and great care was bestowed upon the colouring. His zeal for the task he had undertaken, led him with two friends, one of whom was Mr. John Hancock—perhaps the best ornithologist now living—to visit Norway and explore its coasts in quest of those many British birds, of the nidification of which nothing was known except that it was not carried on in these islands. This expedition in 1833 to a country hitherto so little explored by Englishmen as Norway, was no small proof of enterprise, and, with the simultaneous attempt, with a like intent, made in Iceland by Mr. G. C. Atkinson, bore good fruit, not merely in its immediate results, but even long afterwards; for it was doubtless the example of these gentlemen[1] that prompted the subsequent exertions of Wolley, Hudleston, Salvin, Tristram, and others; while the successes in recent years of Alston, Harvie Brown, Danford, and Seebohm, may also be traced to the same cause. The influence has even extended to the United States, as witness the explorations conducted by Kennicott, Macfarlane, and their indefatigable successors under the authority of the Smithsonian Institution. The result has been that the true home of almost every species of bird which inhabits Europe at any time of the year has been discovered, and the same with a large number of those which inhabit North America, and thus, of course, has accrued a great gain to ornithology.

Mr. Hewitson, however, did not pretend to foresee this sequel to his enterprise and that of his friends. His aim was far humbler. In his own words:—

"However unimportant in itself the branch of natural history which I have attempted to elucidate, the beautiful and varied objects which compose it are amongst the first to excite the imagination and call forth in boyhood those feelings, that love for nature, which are inherent in us all; and however the cares or the pleasures of after life may have erased those earlier feelings, there are few who have not one day derived pleasurable emotions from their contemplation, and who do not remember those joyous times when, at the first breaking loose from school, they have hastened to the wood and the hedge-row in search of their painted prize."

The "British Oology" was soon out of print and a second edition was called for, which, under the title of "Coloured Illustrations of the Eggs of British Birds," was begun in 1842 (when the author took the opportunity of publishing a Supplement to his former issue) and finished in 1846, while in 1853—only eleven years later—a third edition was demanded. This, completed in 1856, remains unquestionably the best publication on the subject; for, though the plates were not so carefully coloured as in the second edition, the number of species represented, chiefly owing to the discoveries of Wolley, was largely increased. But in the meanwhile Hewitson's taste had turned towards another department. He had begun with his usual energy that wonderful collection of diurnal *Lepidoptera*, and works in illustration of that group, with which his name will be always associated, and by which it will probably be most widely known. His villa at Oatlands, with its beautiful view and charming garden, was a sight not to be forgotten, to say nothing of the glorious contents of his cabinets. Here he passed the last twenty-five years of his life, or more; seldom leaving home, always glad to welcome a visitor whose tastes agreed with his own, and occasionally returning to his old "flame," when he could thereby assist a friend—as witness some of the plates in the earlier volumes of *The Ibis*. The promulgation and subsequent prevalence of the doctrines of evolution, however, greatly disturbed him; and perhaps the only thing that ruffled his temper was to hear that one naturalist after another had embraced what to him seemed a pestilent heresy.

[1] It is fair to mention that in 1830 Hoy began a series of tours into the Netherlands with the same object, and in 1831 Salmon made an egg-collecting voyage to Orkney and Shetland, but the places they visited bore no comparison in remoteness and difficulty of travelling to those above-mentioned.

So firmly did he stand on the ancient ways that he has been often heard to say—and he may have even expressed the sentiment in as many words in some of his writings—that he could not look into one of his insect-drawers without disgust did he not believe in the direct and independent creation of each individual species. At any rate he never lost an opportunity of avowing his hatred of Darwinism, though his opposition to it made no difference in his feelings towards those of his friends who were Darwinians.

It is understood that before his death he had arranged for the ultimate transfer of his magnificent collection of Butterflies to the British Museum, where, according to the terms of the compact, its present condition is to remain undisturbed for twenty years. Mr. Hewitson, who was buried at Walton-on-Thames, had been a widower for many years and left no children. A portion of his very considerable fortune he is said to have devoted to charitable purposes, but a large portion of the remainder to his old and tried friend, Mr. John Hancock, while his copyrights go to his publisher, Mr. Van Voorst. It is believed also that Mr. Kirby is to make a catalogue of the collection of *Lepidoptera* before it is removed to the British Museum. A. N.

ANDREAS VON ETTINGSHAUSEN

WE regret to record the death in Vienna, on May 25, of Baron von Ettingshausen, one of the oldest of European physicists. He was born in Heidelberg, November 25, 1796. After the completion of his academic studies, he entered the philosophical faculty of the Vienna University as privat-docent for physics and mathematics in 1817. Two years later he accepted the professorship of physics in Innsbruck, but was called back in 1821 to Vienna, to the chair of mathematics, which position he exchanged in 1834 for the professorship of physics. In 1852 he accepted the direction of the newly-grounded Physical Institute, completed its organisation, and raised it to its prominent position as a centre of physical investigation. Some years since he was compelled by increasing age to retire from the duties of his professorship, after a half-century of unwearied activity.

As an investigator Ettingshausen was first known by his mathematical contributions. In 1834 he was one of the first to apply Faraday's discovery of electric induction; and the magneto-electric machine devised by him at this time, and bearing his name, marks an important step in the progress of this branch of physics. Of his later researches we would mention those on the movements in homogeneous systems of molecules, on the parallelogram of forces, on the law of isochronism in the vibrations of the pendulum, and on the formulæ for the intensities of reflected and refracted light, in all of which the mathematical element was predominant.

Ettingshausen's literary work was confined chiefly to his "Vorlesungen über höhere Mathematik," which appeared in 1827; his "Lehrbuch der Physik," published in 1844, and to the editorship of the "Zeitschrift für Physick und Mathematik," from 1826-1832.

As a lecturer Ettingshausen was one of the leading celebrities of the Austrian capital. His auditorium was thronged not only by the students but by the educated classes of Vienna, who were attracted by his rare combination of oratorical power and experimental elegance.

In the Physical Institute he rendered services of the greatest value. For a number of years Vienna was unexcelled in the opportunities it offered to young physicists, and the present activity in physical research existing throughout the Austrian universities is undoubtedly due in a great measure to the healthful impulse given by Ettingshausen a score of years since. It is probably to the same source that we can trace the marked mathematical character of the modern school of Austrian physicists, nearly all of whom have been trained under his eye.

Ettingshausen's varied services made him the recipient of numerous decorations, and some years since he was raised by the Emperor into the nobility. He was a leading member of the Vienna Academy of Sciences, which he assisted to found, and for a long series of years its general secretary. His researches appeared chiefly in its *Sitzungsberichte*. He leaves behind him a son, Baron Constantine v. Ettingshausen, the well-known authority on palæontology.

A NEW CRATER ON THE LUNAR SURFACE

WHEN examining the surface of the moon on May 27, 1877, Dr. Hermann J. Klein, of Köln, observed, with his $5\frac{1}{2}$-inch dialyte by Plössl, a great black crater on the Mare Vaporum, and a little to the north-west of the well-known crater Hyginus. He describes the crater as being nearly as large as Hyginus, or about three miles in diameter, and, being deep and full of shadow, and as forming a conspicuous object on the dark grey Mare Vaporum. Having frequently observed this region during the last twelve years, Dr. Klein felt certain that no such crater existed in this region at the time of his previous observations. Dr. Klein communicated his observations to Dr. Schmidt, of Athens, the veteran selenographer, who assured him that this crater was absent from all his numerous drawings of this part of the lunar surface; neither is it shown by Schroter, Lohrmann, nor Mädler, who carefully drew this region with the fine refractor at Dorpat. On one or two subsequent occasions Dr. Klein obtained further observations of this new crater. He found it to be either without a wall or with a very low one, but to be a deep conical depression in the surface. Shortly after sunrise the crater takes the appearance of a dark grey spot, with an ill-defined edge.

In April, 1878, Dr. Klein communicated his observations to the editor of the *Selenographical Journal*, who at once took the proper steps to have this object observed by the members of the Selenographical Society. The day for observing this region was unfortunately cloudy, and no observations could be made in England, but Mr. J. Ward, of Belfast, caught a glimpse of the moon through a temporary break in the clouds. He at once saw the crater in the position assigned to it by Dr. Klein, and described it as being a black crater with a soft edge. The next opportunity for observing this crater was May 9, but the occasion was not favourable, the sun being then high above the horizon of this part of the moon. The day turned out cloudy. Messrs. Backhouse and Neison observed through thin clouds, and saw in the position of the new crater a dark elliptical spot. On May 11 Messrs. Knott, Neison, and Sadler observed in this place a dark ovoid mark or shading. So far, then, the English observations have been perfectly in accord with those of Dr. Klein, although bad weather has rendered it impossible to see the new crater as a crater.

Mr. Neison repeatedly examined and drew this portion of the lunar surface during the years 1871-1875, and discovered a number of minute details in the region where Dr. Klein has seen the new crater. Quite close to this object are a number of much smaller craters, several under a mile in diameter. Several of these are shown by Schroter, Lohrmann, Mädler, and Schmidt. It may be regarded, therefore, as absolutely certain, that previous to 1876 there did not exist on this portion of the lunar surface a deep black crater of three miles in diameter, and it is thus Dr. Klein describes the new object seen by him. Mr. Neison has expressed the opinion that it is most improbable that he could have missed seeing so conspicuous an object as the present dark marking which it is certain exists now in this region. If, therefore, the existence of

Dr. Klein's new crater be confirmed, it will form the strongest possible evidence of a real change on the surface of the moon, a change, moreover, of a volcanic nature.

The Mare Vaporum in which the new crater is situated lies close to the centre of the visible surface of the moon, so that objects in this region are very slightly affected by the lunar librations. Fortunately it is a portion of the surface which has been most carefully studied by Lohrmann, Mädler, Schmidt, and Neison; for had this new crater of Dr. Klein appeared in a less well-known region, much doubt would have been felt as to whether it had previously existed or not.

DEEP-SEA DREDGING OFF THE GULF OF MEXICO

THE last number of the *Bulletin* of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass., contains a letter from Alex. Agassiz to the superintendent of the United States Coast Survey, detailing the results of some recent dredging operations in the United States schooner *Blake*. A series of deep-sea dredgings were made in the first place across the Florida Channel from Havana to Sand Key, out to the Tortugas reefs, then across the Gulf to the Yucatan Bank, to Vera Cruz, about the Alacran reef and then across the Yucatan Channel, and in the trough of the Gulf Stream to Sand Key, Florida—in all about 1,100 miles of lines taking the shortest distance from point to point. The results of the cruise are full of interest; we can only allude to a few of them. The great Alacran reef is an atol—an atol existing not as Darwin suggests to be the case with atols in general, in an area of depression, but in one of elevation, like those in which the Florida and Bahamas reefs are found. The formation of the Alacran reef is in full activity, the eastern slope is nearly perpendicular, rising to a height of twenty fathoms from the surface in a comparatively short distance. It is exposed to the full force of the north-east trades and the surf breaks heavily against the great masses of *Madrepora palmata*, which build up the narrow line of coral barely flush with the level of the sea. The western slope is much more gentle, and here the reef consists of a number of half-made narrow islands. These are mere strips of sand formed by the breaking-up of the exposed masses of coral, which are gradually cemented together by the accumulation of the loose material held in suspension by the water. Here, in the shallower parts, grow huge masses of Astræa, of Gorgoniæ, of Mæandrina, which now and then rise to the surface.

Along the Cuban coast the dredge brought up immense numbers of siliceous sponges, a species of Favosites, which we are tantalisingly told is perhaps the most interesting coral ever dredged. We presume it was found living, and we all know that this genus was founded by Lamarck for some fossil corals, only found in the very oldest strata (Silurian and Devonian), a young Holopus in excellent condition (probably the fourth or fifth specimen ever found). The dredge worked well to a depth of upwards of 2,000 fathoms. One haul in 860 fathoms brought up an unusually large number of two and one valved mollusca, including many of exquisite beauty. Some most gorgeously coloured crustacea were brought up from a depth of 1,920 fathoms, and what are we to say to an isopod allied to Aega, and upwards of eleven inches in length and three in width? Amongst the strange fish, we read of one like a huge tadpole with a gigantic round cartilaginous head, and without eyes; of another with a drawn-out flat head, very little eyes, but possessed of gigantic filaments, as long as the whole body, and extending from the tips of the pectoral and lower caudal fins. Some of the Holothurians were striped with bands of a deep crimson colour.

Certainly the wonders of the deep-sea are not yet exhausted, and though the treasures found by our own *Challenger* expedition were great, it could reap the produce of but a very narrow belt out of the great expanse of the ocean world.

A steel wire rope was used by Capt. Sigsbee. The time required to reel in was always below one minute per 100 fathoms, sometimes not more than twenty seconds, while the time required to strike bottom averaged thirty-five to forty-five seconds per 100 fathoms in the deepest soundings of 2,000 fathoms. The wire rope was of galvanised steel with a hemp coil; it measured $1\frac{1}{8}$ inch in circumference, and weighed 1 lb. to the fathom, and had a breaking strain of over 8,600 lbs., and its own weight made the use of heavy weights to sink it unnecessary.

The *Blake* is now on a cruise to explore the inner portions of the Gulf of Mexico, commencing with a run from the Tortugas to the mouth of the Mississippi River, in which we wish her crew of all ranks every success.

E. PERCEVAL WRIGHT

METEOROLOGICAL NOTES

MR. ELLIS has made a valuable contribution to the diurnal variation of the barometer in a paper published in the *Journal* of the Meteorological Society of London, which gives the hourly variations from the means of each month as deduced from a discussion of the photographic records taken at the Royal Observatory during the twenty years ending 1873. The forenoon maximum occurs from May to July about 9 A.M., being fully an hour later than at Kew. The morning minimum at the same season becomes less marked than at other times of the year, as happens in situations more or less continental in middle and higher latitudes; and this feature of the diurnal variation is, it may be remarked, decidedly better marked at Kew than at Greenwich. Mr. Ellis gives, for comparison with Greenwich, the curves for Oxford, Washington, Cape of Good Hope, and Ascension, from which he draws the broad conclusion that in high latitudes the forenoon maximum occurs earlier when the sun rises early, it being however omitted to be pointed out that this holds good only in situations more or less continental or removed from the more immediate influence of the sea. Thus the forenoon maximum which occurs at Greenwich at 9 A.M. and at Kew at 8 A.M., is delayed at Falmouth and Valentia to about 11 A.M. or noon; whilst at Helder the time of its occurrence in June is about 2 P.M. The hourly barometric values for the twenty years were arranged with reference to the time of the moon's meridian passage with the result that no certain indication of lunar variation was apparent. We hope that by-and-by the main details of this elaborate discussion will be printed; such details as will embrace, at least, the hourly values of each day and month of the twenty years for the examination of many inquiries referring to both civil and lunar days, which are now rising into questions of the highest importance.

PROF. LOOMIS has recently examined all the cases of violent winds of the United States which have been recorded as having occurred from September, 1872, to May, 1874, the number of cases on which the wind rose to or exceeded forty miles an hour being 250. During the six months from November to April, violent winds were more than five times as frequent as during the other six months of the year. The great preponderance of violent winds are from the north; thus from north-east, north, and north-west, the number were 143, whereas from south-east, south, and south-west, there were only 58. Generally speaking, violent winds increase in frequency and intensity over North America with latitude. Local conditions exercise a considerable influence on the force of the wind. Thus violent winds are of most frequent occurrence near the Gulf of St. Lawrence and the Great Lakes, particularly Lakes Michigan and Erie.

High winds are also frequent along the dry prairie region bordering on the Rocky Mountains. An interesting table is given, showing the relation between the wind's velocity and the barometric gradients, which may be accepted as rough approximations, but the stations in the United States and in Canada from which the isobaric lines have been drawn, are by far too wide apart from each other to supply the data required in dealing with this important phase of weather.

UNDER the heading of a "Brief Sketch of the Meteorology of the Bombay Presidency in 1876," Mr. Fred. Chambers introduces some original suggestions regarding the Indian drought of that year. His method of examination proceeds on the supposition that the droughts of India may be connected with the varying states of the sun's surface as regards temperature, and in the light of the consequences which result from this supposition the observations made in the presidency, from Kurrachee, in the north, to Belgaum in the south, are discussed from which it is shown that the abnormal barometric movements of 1876 were mainly variations in the intensity of the usual seasonal movements; and that, as regards the rainfall, among the causes which produced the drought in 1876, were those very causes which in ordinary years produce a less average rainfall in the eastern than in the western districts of the Presidency. The general conclusion which is drawn, explicitly for the guidance of further inquiry, is that the same principles which explain the usual alternation of the seasons, also explain in a great measure the varying rainfalls of different years. This mode of discussion deserves to be widely adopted in dealing with secular variations in meteorological phenomena, particularly in view of the large scientific issues involved in the relations between the solar and terrestrial atmospheres.

PROF. MOHN publishes a brief account of the fall of the volcanic ash which was shot up into the air during the eruption in Iceland on March 29, 1875 (NATURE, vol. xi. p. 514), and thence carried eastward by the strong westerly winds which then prevailed; and a map is given showing by curved lines the hours at which the ash began to fall along the extensive track stretching 980 miles from Iceland to Stockholm. The most interesting point in the inquiry is the manner in which the lofty mountain range of Scandinavia appears to have influenced the hour of the fall of the ash. Since the time between the ash leaving Iceland and falling on the coast to the east of Stockholm was twenty-seven hours, the mean rate at which it was borne onward was thirty-six miles per hour. During the first twelve hours of its course it drifted eastwards at a uniform rate of fifty miles an hour. It had then approached to from sixty to eighty miles of the mountains of Norway, but thereafter its speed suddenly fell from fifty to twenty-seven miles an hour. The interesting point is that a mountain system such as that of Norway, lying across the wind's path, would appear to exercise a decided influence in reducing the velocity of the aerial current under its level to the extent of nearly one-half, at a distance of sixty to eighty miles to windward.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSES OF MAY 16, 1882, AND AUGUST 18, 1887.—There will be two total eclipses of the sun within the next ten years, which may be observed without entailing a long sea-voyage from this country. The first will take place on May 16 (or May 17, civil reckoning), 1882. In this eclipse the central line commences in long. 3° 11′ W., lat. 10° 40′ N.; totality will occur with the sun on the meridian in 63° 44′ E. and 38° 35′ N., and at sunset in 138° 51′ E. and 25° 25′ N. The duration of total eclipse on this occasion is comparatively short. Probably if observers proceed from England to the central line, they would station themselves in Upper Egypt, not far from one of the points whence the late transit of Venus was successfully observed. In 32° E. and 26° 44′ N. close to this line, totality commences at 20h. 32m. 45s. and continues 1m. 10s. At a point upon the same, not far from Sherm, at the extremity of the peninsula of Sinai, on the Gulf of Akaba, in 34° 28′ E., and 28° 2′ N., the duration of totality is 1m. 17s. The eclipse will be total at Teheran for 1m. 4s. with the sun at an altitude of 67°, commencing May 16, at 22h. 36m. 10s. local mean time; the central line passes about fifteen miles south of this place. A total eclipse may also be witnessed, though for a few seconds only, near Shanghai.

The second of the eclipses to which reference has been made is the one long mentioned in our popular treatises as the next eclipse that will be total in England, but the central line commences in Germany. The following are the elements:—

G.M.T. of conjunction in R.A., 1887, August 18, at 17h. 14m. 33s.

	°	′	″	
R.A.	148	7	45·2	
Moon's hourly motion in R.A.		36	38·2	
Sun's ,, ,, ...		2	19·5	
Moon's declination	13	33	10·1	N
Sun's ,, ...	12	54	5·2	N
Moon's hourly motion in declination		9	13·0	S.
Sun's ,, ,, ...			48·8	S.
Moon's horizontal parallax		60	12·4	
Sun's ,, ,, ...			8·75	
Moon's true semi-diameter		16	24·4	
Sun's ,, ,, ...		15	48·9	

The central eclipse begins at 16h. 10·2m. in 11° 39′ E. and 51° 38′ N., and ends in 173° 47′ E. and 24° 32′ N., and the eclipse is central with the sun on the meridian in 102° 15′ E. and 53° 46′ N.

The following are also points upon the central line:—

° ′	° ′	° ′	° ′
21 36 E.	53 50 N.	34 15 E.	56 5 N.
24 9	54 21	38 23	56 40
29 19	55 17	42 1	57 7

It will be seen that the track of the eclipse is chiefly over Russian territory. In the longitude of Moscow and in latitude 56° 33′, totality will continue 2m. 30s.; in Moscow itself the eclipse will also be total, though for about fifty seconds only, commencing at 18h. 44m. 40s. mean time, with the sun at an altitude of 17°. By a direct calculation for Berlin a total eclipse for 1m. 40s. results, but the sun is barely at an altitude of 3° when it ends. It is likely that a person wishing to view the phenomena of a total eclipse with the sun at fair elevation, will find Moscow, or its vicinity, the most accessible position; we will therefore append formulæ by which the times of beginning and ending of totality in this neighbourhood, as also the track of the central eclipse, and the north and south limits of totality may be determined, and will also take the opportunity of illustrating the process by a further example, seeing that many experience difficulty in the application of such formulæ. Following the methods described by Mr. W. S. B. Woolhouse in his excellent paper on the calculation of eclipses, transits, and occultations, in the Appendix to the *Nautical Almanac* for 1836, with slight change of notation, we have from the above elements for computation of beginning and ending of total phase at any place not far from Moscow:—

$$\text{Cos. } w = +\ 58{\cdot}7257 - [1{\cdot}92757] \text{ sin. } l + [1{\cdot}43336] \text{ cos. } l. \text{ cos. } (L - 75^\circ\ 51'{\cdot}8).$$

$$t = 17\text{h. } 32\text{m. } 29{\cdot}6\text{s.} \mp [1{\cdot}87565] \text{ sin. } w - [3{\cdot}11123] \text{ sin. } l.$$
$$- [3{\cdot}81636] \text{ cos. } l. \text{ cos. } (L - 83^\circ\ 34'{\cdot}5).$$

And for determination of latitude of central line, and of north and south limits of totality in any assumed longitude not far from that of Moscow, putting

$$n \text{ . sin. } N = +\ [1{\cdot}92757]$$
$$n \text{ . cos. } N = +\ [1{\cdot}43336] \text{ cos. } (L - 75^\circ\ 51'{\cdot}8).$$

We have

$$n \,.\, \cos (N + l) = \begin{cases} -[1·77616] & \text{for N. limit.} \\ -[1·76883] & \text{for central eclipse.} \\ -[1·76137] & \text{for S. limit.} \end{cases}$$

In these formulæ, as has been previously explained when presenting similar ones, all quantities within square brackets are logarithms; l is the *geocentric* latitude, or the geographical latitude diminished by the angle of the vertical; L the longitude from Greenwich, counted positive towards the east; and t results in mean time at Greenwich.

First, let it be required to find the latitude of the central line and the north and south limits in the longitude of the Observatory at Moscow, 2h. 30m. 17s., or 37° 34′·3 east of Greenwich.

Longitude	+37° 34′·3
Constant	−75 51·8
A	−38 17·5
n. sin. N	+1·92757
Constant	+1·43336
Cos. A	+9·89480
n. cos. N	+1·32816
Tan. N	+0·59941
N	75° 52·9
Sin. N	+9·98668
n	+1·94089

For Central Line.

Constant	−1·76883
n	+1·94089
Cos. (N + l)	−9·82794
N + l	132° 17′·4
N	75 52·9
... ...	56 24·5
Add angle of vert...	10·6
Lat. of central line.	56° 35·1

For North Limit.

Constant	−1·77616
n	+1·94089
Cos. (N + l) ...	−9·83527
N + l	133° 11·0
N	75 52·9
l	57 18·1
Angle of vert. ...	10·5
Lat. of N. limit ...	57 28·6

For South Limit.

Constant	−1·76137
n	+1·94089
Cos. (N + l) ...	−9·82048
N + l	131° 24·5
N	75 52·9
l	55 31·6
Angle of vert. ...	10·8
Lat. of S. limit ...	55 42·4

In this manner by assuming other longitudes near that of Moscow we trace out the belt of totality.

Next, to find the times of beginning and ending of the total phase at any point in the vicinity. Calculating for the observatory of Moscow, the geographical latitude of which is + 55° 45′·3, we proceed thus:—

Geographical latitude...	+55° 45′·3	Constant ...	−23° 34′·5
Angle of the vertical ...	10·7	L	+37 34·3
Geocentric latitude (l)..	+55 34·6	B	+13 59·8

Constant	−1·92757
Sin. l	+9·91639
	−1·84396
No.	−69·8167
	+70·7604
Nat. cos. w	+ 0·9437
Log. cos. w	+9·97483

Constant	+1·43336
Cos. l	+9·75228
Cos. A	+9·89480
	+1·08044
No.	+12·0347
Constant	+58·7257
	+70·7604

Constant	1·87565
Sin. w...	9·51962
	1·39527
No. ...	24s·8

Constant	−3·11123
Sin. l ...	+9·91639
	−3·02762
No. ...	−1065s·6
	−3593s·8
	−4659s·4

Constant	−3·81636
Cos. l ...	+9·75228
Cos. B...	+9·98691
	−3·55555
No. ...	−3593s·8

	h.	m.	s.	
	−1	17	39·4	
Constant	17	32	29·6	
	16	14	50·2	
Long. E.	2	30	17·0	
Middle..	18	45	7·2	Moscow M.T.
			∓24·8	
Totality begins	18	44	42·4	,, ,,
Totality ends.	18	45	32·0	,, ,,

GEOGRAPHICAL NOTES

THE *Japan Gazette* publishes an account of a visit recently paid by a Japanese steamer to the Bonin Islands, about which but little is known. Some eighteen months ago the Japanese took possession of the islands (which are in N. lat. 27°, about 520 miles from Yokohama), and established their head-quarters at Port Lloyd, Peel Island, which is the only harbour in the Bonins. The islands are described as high, rocky, and even mountainous; and the shores are, for the most part, precipitous, and lined with coral reefs. The vegetation is chiefly tropical, palms of various kinds being abundant. Wild goats and pigs abound on all the islands, and deer on one of them. Lemons, sweet potatoes, bananas, Indian corn, &c., thrive there; but the attempt to introduce cocoa-nut trees has not yet proved successful. On the return voyage the steamer visited the outlying Japanese island of Hachijo, which has an area of forty miles, and is said to contain 10,000 inhabitants. It is mountainous, and its sides to a great extent precipitous. At the northern end of the island there is a volcanic peak, rising to a height of 2,800 feet above the sea. The roads on the island are mere narrow and stony paths, and the people are poor. Three-fifths of the population are said to be women. Almost every available spot on the hill-sides in Hachijo is terraced and cultivated, but sufficient rice cannot be grown, so that sweet potatoes form one of the principal articles of food.

AT the meeting of the subscribers to the African Exploration Fund held the other day, a resolution was passed to adopt the route recommended by the Committee, from Dar-es-Salaam, towards the northern end of Lake Nyassa, and thence, if possible, to the south end of Lake Tanganyika. The return journey might be made as far as possible along the valley of Lufigi. As we have already intimated, Mr. Keith Johnston, with whom will be associated another European, will lead the expedition, which will probably leave England in October next.

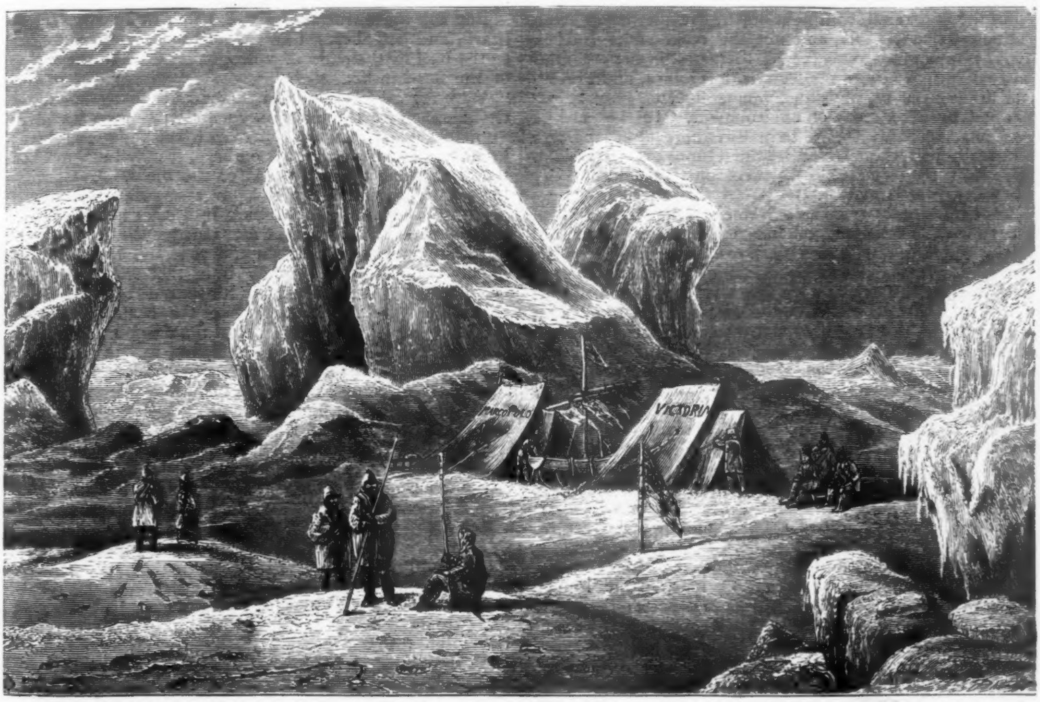
THE distribution of prizes of the Geographical Society of Paris, which had been postponed owing to the forthcoming exhibition, will take place at the Sorbonne on the 27th inst. Mr. Stanley, it is understood, will be present to receive the gold medal awarded him. The National Geographical Congress will take place in the beginning of September in the hotel built by the Paris Geographical Society, and which will be inaugurated on this occasion. It is said on good authority that the presidency of that Congress will be given to M. de Lesseps.

THE GREAT FROZEN SEA[1]

MANY readers, we believe, will prefer this brief brightly written narrative of the last English Arctic Expedition to the two weighty volumes of Sir George Nares's, recently noticed in these pages. Capt. Markham is an enthusiastic Arctic explorer, and as these volumes testify, is well fitted by his personal qualities, his experience, and accomplishments, to take a leading part in work of this kind. He has evidently a thorough knowledge of Arctic work and a full appreciation of the kind of observations which ought to be attended to in an Arctic expedition. His interesting volume affords a very satisfactory idea of the incidents of the expedition and of the nature and amount of work done.

Capt. Markham's name must be known to all as the leader of the sledge party that attained the highest northern latitude, and as might be expected, his pages contain an impressive narrative of the adventures of the party. As one reads the story of this heroic attempt to reach the pole he is not merely surprised that the party turned when they did, but that they did not resign the attempt at the end of the first week, for it must then have become evident that the goal was unattainable by that route at that season with the means at command. Had the men not been made of splendid stuff, physically and morally, they could not possibly have endured the terrible hardships described by Capt. Markham. Certainly Sir George Nares did not exaggerate when, in addressing his men before leaving England, he told them "that if they could imagine the hardest work that they had ever been called upon to perform in their lives intensified to the utmost degree, it would only be as child's play in comparison with the work they would have to perform in sledging."

Capt. Markham seems to think that work by the



Highest northern camp, 33° 20′ 26″ N. lat.

Smith Sound route is practically complete; and he leaves one with the impression that it would be useless to attempt to reach the pole by that route. Assuming that the attainment of the pole is in itself a worthy object for an expedition, we are inclined to think that the conclusion as to its unattainability has been too hastily drawn from the experience of one expedition. At the same time we quite agree with Capt. Markham that there are other routes which, while they hold out some hope of a successful passage to the pole, would also afford opportunities of obtaining valuable scientific observations. Capt. Markham rightly says that Behring Strait is a portal leading to a vast region, the history of which has hitherto been as a sealed book. This, it is stated, is the route to be followed next year by the expedition to be sent out by Mr. Gordon Bennett. Mr. Bennett is having a map of the polar regions constructed for the purpose of showing the effect of the various currents towards and from the polar area, and, if one may judge from this, there is much to say in favour of the Behring Strait route; but all such polar-current charts must be regarded with grave suspicion, as being founded so largely on conjecture. We quite coincide with Capt. Markham's strong advocacy of the route by Franz Josef Land. So far as known at present, that, we think, is the best basis of operations for further work towards the north. Perhaps we may hear of something important being done in this direction by the Dutch Expedition which recently went out in the *Willem Barentz*.

Capt. Markham gives a fair idea of the kind of scientific work carried on by the expedition, and we hope that the many magnetical, hydrographical, meteorological,

[1] The Great Frozen Sea. A Personal Narrative of the Voyage of the *Alert* during the Arctic Expedition of 1875-76. By Capt. A. H. Markham, R.N. (late Commander of H.M.S. *Alert*). (London: Daldy, Isbister, and Co. 1878.)

and other physical observations which were made will be published in well-arranged form. At the furthest point reached, a bread bag filled with the scrapings of the pannikins and a little pemmican was lowered to the bottom of the sea, and, having been kept there for some hours, was hauled up, and was found to be almost alive with numerous small crustaceans and foraminifera. With the thermometer a series of temperatures was taken at every ten fathoms, while the specific gravity of the surface-water was also obtained. Tidal action was apparent, though it was impossible to collect any exact data.

Capt. Markham, like his relative, Mr. C. R. Markham, is evidently of opinion that the Eskimo entered America from Asia, spreading eastward, and finding their way to Greenland by crossing at almost 81° 54'. This is, we confess, the theory which most readily presents itself, but those who have studied the subject most deeply, and in all its aspects, have come to the conclusion that the Eskimo are virtually indigenous, and came northwards from the American continent itself, the migration being from America to Asia, and not the other way. Indeed, some ethnologists go so far as to maintain the essential unity of origin of all the American families, and that all the differences in physique, language, &c., may be explained by differences of environment. In the case of America, probably, more than anywhere else, language is a really important factor in the ethnological problem. (See Prof. Sayce's article last week on "The Ethnology of North-West America.")

Capt. Markham gives an extremely pleasant account of the winter amusements on board the *Alert*—the Royal Arctic Theatre, the Thursday Pops., the school for the men, &c. The last-mentioned institution appears to have been a great success, and we are sure the men will feel the benefit of it all their lives. One feature of the Thursday Pops. we must mention with special approval; except on the evenings exclusively devoted to the legitimate drama, these entertainments were always preceded by a lecture delivered by one of the officers on some interesting and at the same time instructive subject, adapted to the knowledge and intelligence of the audience. In this way thirteen lectures were given altogether, and with the exception of one on a historical subject by Mr. White and one on Sledging Experiments by Capt. Nares, they were all on scientific subjects. Capt. Nares began the series by a lecture on Astronomy, which was followed by lectures by the other officers on Magnetism, Geology, Meteorology, Steam, Mock Moons under the Microscope, Light, Astronomy again, Food in the Arctic Regions, Arctic Plants, Hydrostatics. Indeed it is difficult to conceive that more could have been done to enable the expedition to pass as cheerful a winter as possible under the circumstances.

Altogether Capt. Markham's work is a thoroughly interesting and instructive narrative of a memorable expedition. The numerous illustrations and the maps add considerably to its value.

ON THE STRUCTURE AND DEVELOPMENT OF THE SNAKE

In my paper on the skull of this type (see abstract *Proc.* Roy Soc., January 10, 1878, pp. 13-16) I spoke of the snake as "lying at the very base of the *gill-less* vertebrata, and possessing a skull at once the simplest and yet the most curiously specialised," of any of the many kinds I have worked out.

As far as existing forms of reptiles are concerned, the snake does lie at the *very base*, yet, on the whole, I am inclined to add it to the other limbless lizards, such as the blind-worm and the amphisbæna, and to consider it, therefore, as a lizard which has had its limbs starved out for special purposes.

Much of the cranio-facial axis of the snake remains in a very primordial condition, but the outworks of the skull are modified to such a degree that "the power of nature could no further go."

I have not yet worked out the skull in the amphisbænidæ, but I expect to find it to have many things in common with that of the serpentiform amphibia, the "Cœcilians."

But the "Anguidæ," taking the common blind-worm (*Anguis fragilis*) as an example, are merely "Scincoids" that have dropped their limbs but retained their limb-girdles: they are *lizards* to all intents and purposes, and the native kind only differs from its quadrupedal relatives, in possessing an additional segment ("mesopterygoid") in the "pterygo-palatine" arcade, a segment common in osseous fishes and birds, but suppressed, as a rule, in the scaly reptiles.

As to that which is *archaic*, the chameleons so common in Africa, and the unique New Zealand Hatteria (*Sphenodon*), these outliers of the lizard tribe are evidently more generalised than the serpents.

But all these forms—snake, tropical lizard, legless lizard, and old aberrant lizards—all these come as close to the bird as the *pupa* of a dragon-fly does to the *imago* of the same insect.

With regard to the earlier stages and to the mode of development of the embryo, generally, I have stated in my paper (pp. 9 and 10), that "As to the general embryological study of the snake's embryo, it may be remarked that it is almost exactly that of the birds. Comparing my own observations on this low type with the results given in the study of the chick in Foster and Balfour's excellent work, I find that few paragraphs in it would need any material alteration, and that the figures would mostly serve very accurately if in that work the word *chick*- were to be exchanged for that of *snake*-embryo. The development of the vesicles of the brain, the organs of special sense, the rudiments of the cranium and face—those things that come across my path, to say nothing of the rest of the growing germ, all are developed similarly in the snake, below, and in the bird, above."

If this be so, the modifications undergone afterwards, in the specialisation of the skull and skeleton generally, and in the appearing and packing of the enclothing muscular masses, those "cunning machines" that do the gymnastics of the body—the development and endless modifications of these parts must be of the greatest interest.

I must refer to Professor Huxley's paper "On the Classification of Birds" (Zool. *Proc.*, 1867, pp. 415-418) for a comparison of the bird with the reptile, and for the reasons existing that have led modern anatomical zoologists to put the reptiles and birds into one group, viz., the "Sauropsida." [1]

With regard to the loss of limbs it is not a little remarkable that, on the theory of the "Ratitæ" being parental to the "Carinatæ," in the bird class, that pair of limbs which was to be most metamorphosed was not quickened into new life until it had died. Morphogically, the wingless *Dinornis* stands directly beneath the whole of the "winged fowl" known to us, and the steps and stages from that monster up to the sun-bird and the humming-bird are very gentle and gradual.

But there were *reptiles* in the olden times "that spread their limber fans for wings," and there were true birds also which had evidently only just escaped from the reptilian territory, as the *Archæopteryx*, for instance, and these are seen to be actually modifying the paw into a *wing*.

Perchance the birds grew out from many a kind of old generalised reptile; yet, be this as it may, the eagle himself is not a more powerful or beautiful creature than a python or a boa, nor is there much more to wonder at in

[1] That account of the "Sauropsida" needs a little modification in the light of newer discoveries. I have given such an *improved* account in my article on the Anatomy of Birds in the ninth edition of the "Encyclopædia Britannica," vol. iii., p. 278.

the manner in which the morphological force has enclothed a vertebrated animal in the case of the bird than in that of the huge "creeping thing."

Certainly the skull is in some respects much more simple in the serpent than in the bird, for the bird having built up its skull with all the old reptilian architectural elements, afterwards blots out their distinctness for the most part, and only leaves marks here and there of the early subdivision of the parts.

But this is due to "the hot condition of their blood" and, especially in the higher kinds, the "altrices," the life-vessel of a bird almost literally boils over; in a few short weeks the shapeless embryo of a swallow or a swift is able to join the "aîry caravan" of its migrating parents

Head of Embryo Snake, 1 inch long, magnified 8 diameters.

and relations "high over seas," and in far distant countries seek for perpetual summer.

The great serpent, I ween, took a century or two to finish in its fulness his huge bulk; time, so important to the "turtle, and the crane, and the swallow," could be of no importance whatever to pre-Adamite boa-constrictors and pythons. Was not the whole jungle theirs, and theirs also every kid and fawn, to say nothing of the luckless unwary bird?

That the spinal column is as complete and beautiful a piece of machinery in a boa-constrictor or ordinary snake as in the bird there can be no doubt.

Talk of specialisation! Why, Prof. Owen's terms for the parts and processes of a snake's vertebra would take

Embryo of Snake, ¾ inch, magnified 8 diameters.

up half a column in a scientific glossary. I will give a few of his terms:—"Neural-spine," "neurapophysis," "post-zygaphophysis," "præ-zygapophysis," "zygosphene," "zygantrum," "procœlous," articular cup of "centrum," posterior ball for next cup of "centrum," "neural canal," oval articular head for ribs on each "diapophysis," and oval concavity on head of rib.

Four hundred vertebræ, most of which have all these parts! Surely this creature was made by Nature herself, and by no "'prentice hand."

The sinuous cylindroidal facets, fore and aft, on the bird's centrum are not a whit more perfect than the cup-and-ball of the snake's vertebra; and in all respects the articulation of the serpent's spine is so exquisitely perfect as to beggar all human inventions of joints and hinges.

Only just a little motion of joint on joint is allowed, each joint set into the other, so that nothing can part them without crushing them entirely; and yet a most perfect and delicate motion of cup in ball, wedge in cavity, and of the oblique overlapping facet on the oblique facet beneath it—all these are harmonised together, and just allow a gentle bend of bone on bone, and a gentle rolling of vertebra on vertebra.

Multiply by 400 this limited motion, this arrested curve, and you get a motion such as would, if likely to be applied to you, personally, make "all your safety to lie in remotion, and your best defence absence." The curve, so small as made by one joint bending on another, would, in its sum total, be sufficient to engirdle a luckless anatomist several times over.

In the bird's head nearly all the fair details of its early architecture are plastered over by *periosteal* bone, by the ruthless processes of a steady *ankylosis* that removes landmark after landmark.

Not so in the serpent, although, with a wise prevision (enough to satisfy the most craving teleologist, who, wondering, asks you if you see no *design* in Nature), ankylosis comes in to perfect the "strong box" in which this wise [cunning] creature keeps its limited brain.[1]

The organ of its mind is thus safely lodged so that no foot may crush or wild beast break its casket; thus with its "cruel venom" the adder "bites the horse heels so that the rider falls backwards," and is in no fear of that heaviest of all feet, the foot of the *soliped*.

The (relatively) deaf adder has its ear-organs encased in adamant; they with the cranial bones are "shut up together as with a close seal. One is so near to another that no air can come between them. They are joined one to another; they stick together that they cannot be sundered."

So much for the cranium proper; but how about the *face*?

The *face* is a loose framework of bones tied together into one piece of work by an infinite amount of "yellow elastic tissue," and the opening of the capacious "maw" is surrounded and defended by bars of ivory-like bone, many of which are beset with *retral* teeth pointed like needles and sharp as lancets.

Your serpent, with all his wisdom, does not "mouth" his words; he only hisses; but he *mouths* his prey as no other creature does; and the "shirt of Nessus" was not a more dreadful robe to wear than the distensible body of a python, inclosing, ingulphing, suffocating, and digesting its limp and helpless prey.

With regard to the relation of the snakes to the existing lizards, it is a remarkable fact that, whilst they have no tympanic cavity, in which character they agree with *Sphenodon* and the chamæleons, yet a small *cochlea* buds out from the vestibule, and there is to it a "fenestra rotunda." The chamæleon is void of this structure, and thus in that respect is as low as a frog.

The lower jaw and its pier (quadrate) was altogether directed forwards in the early embryo of the snake; afterwards the pier and the free mandible are articulated at a very acute angle, the squamosal touches the temporal regions by its apex, and to its base the long rib-like quadrate is articulated.

The quadrate thus is made to pass over the "columella auris," which also is directed backwards; on that rod there was a small "stylo-hyal"; the quadrate picks up this *useless remnant*, and glues it, by partial ankylosis, to its inner face.

Thus the counterpart of the human "styloid process" is ankylosed to the bone that answers to the *head* of the "malleus."

W. K. PARKER

[1] I am frequently asked whether I believe in *design*, and am always at a loss how to answer the question, it seems to be to me so perfectly gratuitous. If the questioner would but give me time, I would promise to write him a book upon the fitnesses to be seen in a *frog* or even in a *flea* that should be as large as a family Bible.

A FOSSIL SPARROW-LIKE BIRD

WE recently referred to a new genus and species of Passerine bird, described by Mr. J. A. Allen from a specimen found preserved in the insect-bearing shales of Florissant, Colorado. We give an illustration of these remains, which consist of the greater part of a skeleton, embracing all of the bones of the anterior and posterior extremities (excepting the femora). Unfortunately, the bill and the anterior portion of the head are wanting, but the

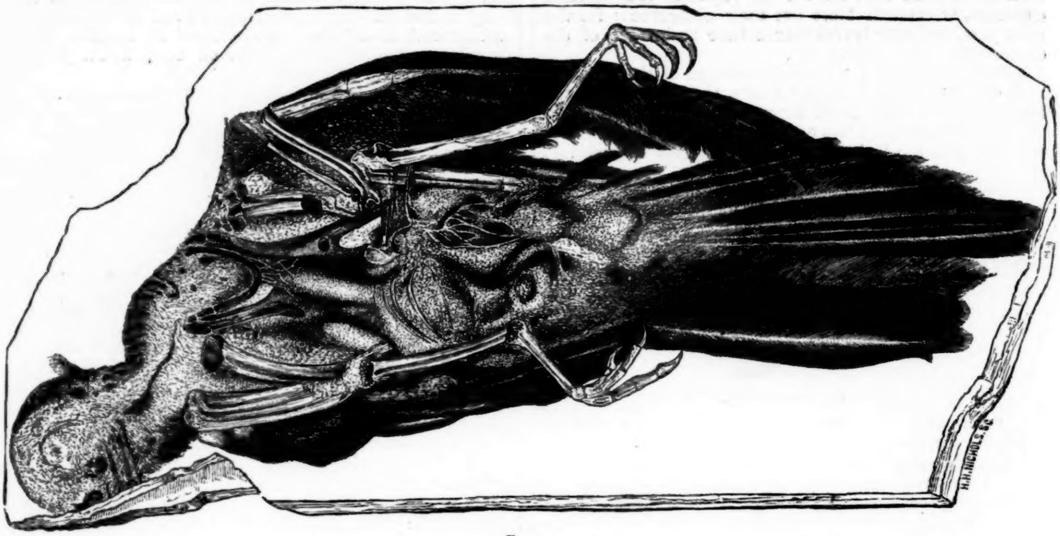


FIG. 1.

outlines of the remainder of the head and of the neck are distinctly traceable. The bones are all *in situ*, and indicate beyond question a high ornithic type, probably referable to the Oscine division of the *Passeres*. The specimen bears also remarkably distinct impressions of the wings and tail, indicating not only the general form of these parts, but even the shafts and barbs of the feathers.

In size and in general proportions the present species differs little from the Scarlet Tanager (*Pyranga rubra*) or the Cedar-bird (*Ampelis cedrorum*). The bones of the wings, as well as the wings themselves, indicate a similar alar development, but the tarsi and feet are rather smaller and weaker; and hence in this point the agreement is better with the short-legged Pewees (genus *Contopus*). These features indicate arboreal habits and well-developed powers of flight. The absence of the bill renders it impossible to assign the species to any particular family, but the fossil on the whole gives the impression of Fringilline affinities.

It is called *Palæospiza bella*. Its wings are rather long and pointed; the tail is (apparently[1]) about two-thirds the length of the wing, rounded or graduated, the outer feathers (as preserved) being much shorter than the inner. The feet and toes it will be seen are strictly those of a perching bird, and the proportionate length of the bones of the fore and hind limbs is the same as in ordinary arboreal *Passeres*, especially as represented by the *Tanagridæ*.

The most remarkable feature of the specimen is the definiteness of the feather impressions. Both the shafts and the barbs are shown with great distinctness in the rectrices, and the tips of the primaries of one wing are also sharply defined, overlying the edge of the partly-expanded tail. The tip of the opposite wing can also be seen beneath the tail. The feet are so beautifully preserved that even the claws are perfectly distinct (Fig. 1).

[1] The character of the tail must be given with reservation, since it is not quite certain that the whole of the tail, or that the exact form of the terminal portion, is shown, especially as the preserved impression is somewhat unsymmetrical.

Another very imperfect specimen from the same locality, and probably representing the same species, consists of the tip of the tail and about the apical third of a half-

FIG. 2.

expanded wing (Fig. 2). In this example the tail is also pointed and graduated.

The larger specimen, that first described, is divided into

an upper and a lower half, the greater part, however, adhering to the lower slab. The bones adhere about equally to the two faces. The drawing is made from the lower slab, with some of the details filled in from the upper one. The feather impressions are about equally distinct on both, and where in either case the bones are absent exact moulds of them remain, so that the structure can be seen and measurements taken almost equally well from either slab.

The species here described is of special interest as being the first fossil Passerine bird discovered in North America, although birds of this group have been known for many years from the tertiary deposits of Europe.

The author is indebted for the opportunity of describing these interesting specimens to Mr. S. H. Scudder, who obtained them during his last season's (1877) explorations of the Florissant insect-beds. The specimens are now the property of the Boston Society of Natural History.

NOTES

A TELEGRAM from Sydney, dated June 17, announces the death of the Rev. W. B. Clarke, the eminent Australian geologist. Mr. Clarke was a Fellow of the Royal Society.

AT Gotha a monument erected in memory of the well-known naturalist, Prof. Johann Friedrich Blumenbach, who died at Göttingen, in 1840, was unveiled on May 19. It consists of a gigantic block of stone bearing a portrait of Blumenbach and an inscription, and was executed after the design of the eminent architect, Herr Eelbo.

THE next session of the French Association for the Advancement of Science will be held at Paris from August 22 to 29. The presidents of sections have been appointed by the general committee. Among them we find the names of MM. Cornu, Quatrefages, Bertillon, Maunoir, Wurtz, Hervé-Mangon, Baron Thenard. It is stated that for the first time each of these presidents will deliver an introductory address on the work of his section, after the example of the British Association.

Two Japanese demonstrations Janagi and Issono, are busily engaged in studying the equipment of our European observatories, and the best methods of conducting observations. At present they are visiting the Seeberger observatory at Gotha. After an extensive summer tour they intend to spend the autumn in Berlin, a city for which Japanese students in various branches of science seem to have a peculiar liking.

THE scientific promenades, which we announced as being organised in connection with the Paris Exhibition, were commenced on June 17 by the Anthropological Commission. Scientific explanations will be given four times a week, from ten o'clock by three professors of the Anthropological School of Paris: Monday and Thursday on Prehistoric Anthropology by M. de Mortillet; Tuesday, on Demography, by Dr. Bertillon; and Friday, by Dr. Topinard, on General Anthropology. The General Association for Lectures and Promenades has been authorised by the Minister of Public Works to complete its organisation, and its programme will be published soon. No fee is taken beside the charge of the usual admittance ticket, 10 deniers, collected at the gates of the Exhibition.

THE Committee of the Meteorological Congress, which will take place in Paris at the end of August, under the presidency of M. Hervé-Mangon, have issued their programme of questions.

THE first of the International Congresses arranged for by the French Government has taken place at the Trocadero. The Société des Agriculteurs de France took the initiative under the presidency of the Marquis de Dampierre, the Prince of Wales and Lord Lyons being present. But the attendance was very limited, not more than five or six hundred persons being present in a room fitted to accommodate many thousands. The number of delegates of French and foreign agricultural associations was 112, a large proportion belonging to English societies. The General Secretary delivered an elaborate address in which he reviewed the condition of agriculture in the world generally and principally in England, which may be considered as the home of modern scientific agriculture. The ordinary meetings of the Congress take place in the Pavillon de Flore, Tuileries, and the concluding sitting will be held in the large hall at the Trocadero. The same organisation has been adopted for all the congresses belonging to the Exhibition. The *Journal Officiel* has published their dates and details of organisation.

THE Paris Prefect of Police has granted the authorisation for the creation of a club of students (Cercle des Écoles). This institution is organised by a committee of *bonâ fide* students and professors of the several Government schools and universities, among them being MM. Littré, Hervé-Mangon, Acarias, Wurtz, Robin, Paul Bert, &c., &c. The Minister of Public Instruction has sent his approbation. Social, political, and religious discussions will be strictly forbidden in the institution. It is the first time, at least during the present century, that such an authorisation has been given in Paris.

WE learn, with pleasure, that at a meeting held at Barrow-in-Furness, on June 3, the Committee of the Naturalists' Field Club belonging to that town determined to organise a scheme for sending representatives (artisans, if possible) to the Paris Exhibition, with the view of collecting information in connection with the various branches of science which are there practically illustrated, one of the conditions being that the result of the observations should be imparted to the club in the form of lectures during the ensuing winter. Promises of substantial support have been received from several of the leading men in the district, and the scheme is expected to be shortly in working order.

WE have often had occasion to refer to the progress of science in New Zealand. Our contemporary, *The Colonies and India*, has, in a recent number, an article on education in New Zealand, from which we gather the following facts:—It seems that upwards of 600,000 acres of land is now set apart to provide funds for these educational establishments. Our contemporary may well ask, "Compared with this, what are the endowments made in this or in any other country in the Old World? What may not be hoped from such a commencement, and from a people possessed of such foresight and liberality?" There is a university established with a Royal Charter whose degrees are recognised as equal to those of the English universities. As yet it is only in its infancy. Having no examiners of its own it has still to conduct the examinations for degrees, through means of the professional staff of the colleges which are affiliated to it. The Canterbury College is thus united to it, where the course i cludes classics, mathematics, modern languages, history, English literature, natural philosophy, political economy, and jurisprudence. This college has received as an endowment 350,000 acres of land, judiciously selected in various districts, and producing a rental of several thousands per annum. In the course of years this will no doubt prove to be of enormous value. "It is open to purchase, at any time, at the rate of 2*l*. an acre; 700,000*l*. is therefore the maximum at which this endowment can arrive. In addition to this there is also a landed endowment for educational purposes, including not only the elementary schools but those of technical science, for classics and superior education, a museum and library, a college of agriculture, and a normal school for the instruction of teachers, a most useful idea." Be ides these there is the

Canterbury museum and public library, and various similar institutions in the country towns. Lectures are given in the museum; and it is hoped that in course of time the library will become as large, or at least as useful, as those of Melbourne and Boston. Twenty scholarships of 40*l.* a year, tenable for two years for students of schools, colleges, or under private tuition, have already been founded by the Board of Education, and it is intended to increase the number. At Dunedin, the capital of Otago, which is chiefly a Scotch settlement, the same eagerness for education prevails. There is a university and a school of art, a boys' and girls' high school, and district grammar schools; besides which there are athenæums and public libraries in nearly all the country villages. "Here, as at Canterbury, large landed endowments have been made for the above-named objects. Two hundred thousand acres have been settled upon the university. The buildings have already cost 30,000*l.*; they are handsome and well-situated. As yet the number of students does not exceed eighty, to instruct whom there are five professors in addition to one of moral and mental philosophy, endowed with 600*l.* a year by the synod of Otago. A valuable library is attached, which it is intended shall be utilised as a free public library. Although this has been styled a university, it can only be looked upon as a college affiliated to the University of New Zealand. A Royal Charter has been refused to it, and its degrees are not recognised. Nearly one thousand of the elder pupils at the other schools receive, at the school of art, instruction in freehand drawing, painting from copies, from nature, and from the human figure, designing, practical geometry, perspective, mechanical and architectural drawing. In the provinces of Wellington, Nelson, and Auckland there are collegiate bodies affiliated to the University of New Zealand, and there are also provisions for elementary instruction. The general dissemination and desire for knowledge, it is said, is "laying a sure foundation of a people able to conduct their own affairs, and giving promise of a bright future in what has well been termed the Great Britain of the south."

WE understand that Mr. Thomas Denman, Lecturer on Physiology at the Birkbeck Institution and Physical Science Lecturer at the Working Men's College, has compiled a Glossary of Biological, Anatomical, and Physiological Terms, which will shortly be published in a small crown 8vo volume by Messrs. Griffith and Farran.

THE Chinese coast was visited by a terrific cyclone on April 12. It appeared to take its origin about fifty miles from Macao, and moved directly northwards, devastating everything within a path of about 700 feet in width. The European settlement on the Island of Schameen was reduced to a ruin, and the havoc created by the storm in Canton and the neighbourhood is beyond calculation. The loss of life is estimated at 6,000 to 8,000. An eye-witness states, in a letter to a Vienna journal, that the cyclone was immediately preceded by a hail-storm, the temperature being at 80° F.

MR. TALFOURD ELY, the Secretary of University College, London, asks us to state, to prevent misunderstanding, that the admission of women to classes in that College does not apply to the Faculty of Medicine, but only to the Faculties of Arts and Law, and of Science.

DURING the past year the Austrian Educational Department has maintained a party of geologists in Northern Greece for the purpose of preparing a reliable geological chart of this part of the kingdom, a district which, until within late years, has been almost entirely closed to scientific examination. A portion of the results have been submitted to the Vienna Academy recently in the form of a paper on the "Geological Structure of Attica, Bœotia, Locris, and Parnassus," accompanied by a number of barometric measurements of the heights of Greek mountains.

IN 1866 the Swiss government took active measures to preserve the numerous erratic boulders scattered over the country, and its efforts have been so ably seconded by the cantonal natural history societies that the most important of these silent witnesses to ancient glacial action have been carefully sought out and protected from destruction. The geologists of France have, as we intimated some time ago, lately awakened to the necessity of making a similar provision for the numerous erratic masses in the departments adjoining the Vosges, the Alps, and the Pyrenees, many of the most valuable of which have already been appropriated for building or other purposes. It is but lately that the immensity of the glacial action in eastern France has been comprehended. For the past ten years the two geologists, MM. Falsan and Chantre, have been occupied in a thorough study of the great movements which once took place in the valley of the Rhone. Their results are embodied in six large maps, on a scale of an inch to the mile, which give a careful reproduction of the striæ, marking the progress of glaciers over the rocks in the valley of the Rhone. From their investigations it appears that the ice in the neighbourhood of Grenoble possessed a thickness of over 3,000 feet, and that the glacier formed an enormous fan-shaped mass, bounded on one side by the alps of Savoy and Dauphiné, and on the other by the mountain ranges of Beaujolais and Lyonnais, and extended beyond Thodure. For the careful mapping of the movements of the Rhone glacier not only the abundant heaps of pebbles and the striæ have rendered the chief service, the erratic blocks have at every stage played a most important *rôle*; and it is to be hoped that the efforts now set on foot will preserve to coming geologists the means of thoroughly tracing the paths of the great glaciers in other parts of the country.

IN the last Annual Report of the Prussian Commission for the Scientific Examination of the German Sea-coast, we notice an interesting comparison of the relative results obtained from equal areas of (1) fish ponds, (2) grazing districts in Schleswig-Holstein, and (3) fishing grounds off Hela. The latter covered a surface of 7,200 hectares, and supplied, in the course of a year, 456,000 pounds of fish as the result of 3,405 expeditions. As contrasted with each other, per hectare, the land yielded 167 lbs. of meat, the fish-pond yielded 153 lbs. of fish; and the sea-fisheries yielded 63 lbs. of fish annually. This is the first effort to establish a comparative estimate of the value of fisheries, and affords some idea of the sources of wealth at the disposal of maritime nations, even when contrasted with the adjoining land.

THE Geographical Society of Vienna has conferred the title of honorary member upon Prof. A. Bastian and Dr. Brehm.

On May 16 a meeting of friends of natural history was held at Dresden, when a resolution was passed to found a society for the establishment and maintenance of an aquarium in that city.

THE *Électricité*, a scientific paper which was started two years ago by Count Halley Darroz for promoting a special electrical exhibition, will resume its publication on July 1 next, to promote a similar project to be executed at the Paris Palais de l'Industrie in 1879.

A NEW work on Russia, entitled "Das malerische Russland," is about to be published by B. M. Wolff, of St. Petersburg. The editor is Herr P. Semenow, Chief of the Russian Statistical Department. The work will consist of four volumes, and will contain over 500 illustrations.

M. DE FONVIELLE writes that he has learned by private letter from Philadelphia, and from a design published by the *New York Daily Graphic* that Prof. Ritchel succeeded in directing balloons in the interior of the permanent exhibition building on May 22 last. About the same time M. de Fonvielle witnessed an experiment by Capt. Annibal Ardisson in the Paris hippodrome, which

was successful also so far as demonstrating the possibility of motion; but the apparatus was so imperfect that the balloon moved very slowly indeed, and another apparatus has to be made by the French experimentor. Instead of using common lighting gas, Prof. Ritchel resorted to pure hydrogen. His balloon had only 3,000 cubic feet measurement whilst Capt. Ardisson's wanted about 11,000. Capt. Ardisson's motor was composed of two very imperfect fans worked with the hand. Prof. Ritchel used a screw propeller moved with both feet, so that he had his hands free for working a horizontal fan, for ascending and descending at pleasure. Instead of constructing a spherical balloon, Prof. Ritchel had prepared a cylindrical one similar to the balloon *Delamare* tried fifteen years ago without success, in the open air. It is stated that Prof. Ritchel's success was very great, and the experiment will be tried again in Philadelphia, and probably soon in Paris. These experiments, M. de Fonvielle thinks, disprove the scheme advocated by the head of the French balloon service, Col. Laussedat, who, in a paper recently referred to in NATURE, suggested that the motive power should be applied to the balloon instead of being annexed to the car.

A VALUABLE sketch of the development of the natural sciences in Holland, has lately appeared in Leyden from the pen of Dr. B. van Haan.

THE late investigations of Count Wurnbrand, on the *loess* formations of the Danube in Moravia, lead him to the opinion that these deposits are entirely of an alluvial origin, and not due to diluvial disturbances. A large variety of fragments of charcoal, carved bits of bone and horn, flints, &c., accompanying the collections of animal remains found in these strata, point with great certainty to the existence of mankind at the time of their formation.

AN interesting archæological discovery is chronicled by the Berne papers. A forest in the neighbourhood is found to grow above a buried Roman town. Numerous edifices have been laid bare, and the various remains which have been unearthed show it to have been inhabited by the officers of the Roman forces, who occupied the strong defensive positions on the river Aar.

AMONG the more important scientific novelties in the German book trade during the past month, we notice the following works:—"Die Dolomit-Riffe von Südtirol und Venetien," 1ste Lief., Dr. E. von Mogsisovics (Vienna); "Die Reptilien und Fische der böhmischen Kreideformation," Prof. A. Frie (Prague); "Die Erdrinde und ihre Bildung," J. Lippert (Prague); "Vorträge über Geologie," F. Henrich (Wiesbaden); "Die Geologie und ihre Anwendung auf die Kenntniss der Bodenbeschaffenheit der oesterr.-ungar. Monarchie," F. von Hauer (Vienna); "Exkursionsflora für Mittel- und Norddeutschland," Exkursionsflora für Suddeutschland," Dr. M. Seubert (Stuttgart); "Taschenbuch der deutschen und schweizer Flora," E. Hallier (Leipzig); "Flora von Deutschland," Prof. A. Garcke (Berlin); "Die Schule der Physik," J. Müller (Brunswick); "Grundzüge der Elektricitätslehre," W. von Beetz (Stuttgart); "Lehrbuch der Physik," F. J. Pisko (Brünn); "Sonne und Monde als Bildner der Erdschale," J. H. Schmick (Leipzig); "Ueber Meerströmungen," E. Witte (Pless); "Anleitung zum Experimentiren bei Vorlesungen über anorganische Chemie," Prof. K. Heumann, III. (Brunswick); "Anleitung zur quantitativen chemischen Analyse," Prof. C. R. Fresenius, II. 2 (Brunswick).

WE have upon our table the following books:—"Outlines of Physiology," by Dr. McKendrick (Maclehose, Glasgow); "Choice and Chance," third edition, by W. A. Whitworth, M.A. (Deighton, Bell, and Co., Cambridge); "A Library Map of London and its Suburbs," by J. B. Jordan (Stanford); "A Geological Map of England," by Prof. Ramsay (Stanford); "A Geological Map of Ireland," by Prof. E. Hull (Stanford); "Grundzüge der Electricitatslehre," by Dr. W. von Bertz (Stuttgart); "A Candid Examination of Theism," by Physicus (Trübner and Co.); "A School Flora," by Dr. Marshall Watts (Warne and Co.).

THE additions to the Zoological Society's Gardens during the past week include a Black-faced Spider Monkey (*Ateles ater*) from East Peru, an Ocelot (*Felis pardalis*), a West Indian Rail (*Aremides cayennensis*), a Black Tortoise (*Testudo carbonaria*), a Common Boa (*Boa constrictor*) from South America, presented by Capt. J. Moir; a Himalayan Bear (*Ursus tibetanus*), an Indian Crow (*Corvus splendens*) from India, presented by Capt. J. S. Murray; a Rufous Rat Kangaroo (*Hypsiprymnus rufescens*) from New South Wales, presented by Mr. Thos. Wickenden; Six Herring Gulls (*Larus argentatus*) European, presented by Mr. Arthur Clarke; two Black-crested Cardinals (*Gubernatrix cristatella*) from South America, an American Thrush (*Turdus migratorius*) from North America, presented by Mrs. Arabin; a Black Saki (*Pithecia satanas*) from the Lower Amazons, a Spotted Cavy (*Cœlogenys paca*), a White Ibis (*Ibis alba*) from South America, purchased; a Chimpanzee (*Troglodytes niger*) from West Africa, deposited; a Reeves's Muntjac (*Cervulus reevesii*) born, six Upland Geese (*Bernicla magellanica*), a Brazilian Teal (*Querquedula brasiliensis*) bred in the Gardens.

THE MICROPHONE[1]

A LATE member of the present ministry, at a dinner given by the institution whose hospitality we experience in this hall, implied, on the authority of one of the leading members of the engineering profession, that invention, like cocktails and Colorado beetles, had taken root in America and had deserted old England. It is therefore to me, as I am sure it is to you, a great gratification to have brought before us an invention which is the offspring of British soil. During the last few months the science of acoustics has made marvellous and rapid strides. First of all we had the telephone, which enabled us to transmit human speech to distances far beyond the reach of the ear and the eye. Then we had the phonograph, which enabled us to reproduce sounds uttered at any place and at any time; and now we have that still more wonderful instrument, which not only enables us to hear sounds that would otherwise be inaudible, but also enables us to magnify sounds that are audible; in other words, the instrument which I shall have the pleasure of bringing before you to-night, is one that acts towards the ear in the same capacity as the microscope acts towards the eye.

I may point out, in the first instance, that the telephone and the phonograph depend essentially upon the fact—and a great fact it is—that the mere vibration of a diaphragm can reproduce all the tones of the human voice. In the telephone the voice is also made to vibrate a diaphragm, which, by completing an electric circuit, or by varying a magnetic field, or by altering the resistance or electromotive force of the circuit, produces effects at a distance which result in the reproduction of the motion of the diaphragm. But in this new instrument diaphragms are cast aside, and we have the direct conversion of sonorous vibrations, or sound waves, into forms of electrical action.

Now, if it had been the habit or the custom of this Society to give to the papers and discussions delivered here sensational titles, I should have been inclined to call the few remarks I am going to make to-night, "A Philosopher Unearthed." Prof. Hughes is well known to us all; he has been more or less associated with this Society since its first inception. Whenever he is in London he is amongst us. His instrument is well known to us as one of the most exquisite pieces of mechanism ever invented; and his works, though few, are known because they are sound. The chief characteristic of this philosopher whom I have succeeded in unearthing, is his extreme modesty. If he had been left to himself, I do not think we should ever have had the microphone here; but, by a lucky chance, he admitted me into his secret, and following, as I have done, all his steps, I am

[1] A lecture given before the Society of Telegraph Engineers, on May 23, by W. H. Preece, Vice-President Soc. T.E., Memb. Inst. C.E., &c., &c.

enabled to-night to bring before you the results of his labours, and they have been labours indeed. For months and months he has been working and straining at the ideas which at last he has elaborated into the microphone.

Now the chief characteristic of the apparatus I am going to introduce to you to-night is its great homeliness, its uncouth roughness, and its absurd simplicity. With common nails, with small pieces of wood, with halfpenny money-boxes, with plain sealing-wax, with the ordinary apparatus which every child has at its command, he has been able to attack nature in her stronghold—to ask her questions and receive back answers, and lay bare to us facts and thoughts which, though they have existed from time immemorial, are brought to light now for the first time.

Now, let us in the first place ask ourselves this question: What is sound? It is a very difficult question to answer in the short time at my disposal; but it is necessary that I should first say something to you about the nature of sound, and then say something about the nature of electricity, and show you how the one can be converted into the other.

Now, what is sound? While I am speaking to you I am setting the air in this room into vibration. The air of this room is composed of an infinite number of infinitely small molecules; every molecule is set in motion, and vibrates to and fro, backwards and forwards, like the bob of a pendulum, and between my mouth and every one of the ears in this hall there is a rapid but short excursion to and fro of every single molecule that comprises the atmosphere of this room; and it is the impinging of these molecules against the drum of the ear that produces that sensation called *sound*. But more than that, not only is the air of this room in this marvellous state of motion, but every piece of wood, every wall, every picture, everything in this hall at this moment, is almost, I may say, alive, trembling away, moving backwards and forwards, forming what are called sonorous vibrations. If the sound be loud enough, and the note deep enough, we can distinctly feel these vibrations. Sound is therefore the vibration in particular periods and particular phases of matter.

Now what is electricity? Faraday, the greatest electrician perhaps that ever lived, was asked that question, and he said the more he studied electricity, the more he unravelled its mysteries, the more mystified he became as to its cause and its origin; therefore it seems an act of impudence on my part or the part of any one else to attempt to answer the question, What is electricity? But great strides have been made since the days of Faraday; we know a great deal more now of the internal molecular action of bodies; we know that light, and heat, and sound are the mere action of those molecules of which matter is composed, and we feel sure, from the facts brought to our notice by the delicate apparatus of the present day, that electricity is simply a mode of motion, nothing more or less than the simple play of the molecules of matter. The truth of this will be made evident to-night by the wonderful connection which exists between sound—which we know to be a mere mode of vibration—and electricity, which will reproduce to us the effects of sound. To make this evident to us we must have a detector which will render apparent to us any electrical action that shall result in sound, and it fortunately happens that this marvellous telephone is an instrument of such extreme delicacy that it has made us acquainted with currents of electricity hitherto unknown, though their presence has been suspected. The telephone which Prof. Hughes has employed in his researches is as simple in its construction as all his other apparatus. It consists of two rough pieces of board clamped together. There is half the coil of an electro-magnet that probably has been in his possession since his early experiments to judge from its appearance. The magnet is a piece of steel rod that has been magnetized. The wire used, and which he has found extremely useful, is wire that was originally made for very different purposes, viz., for ladies' bonnets, and in front of this is placed a piece of ferrotype iron, well-known by those who have experimented with the telephone.

But what is the source of sound? It was necessary in making these experiments that he should have a source of sound. His source of sound was a small mantelpiece clock of French manufacture, which cost originally three or four francs. It has been in use many years, and has been in many parts of the world. It is repaired with great lumps of sealing-wax, but nevertheless it has, or ought to have, a pendulum, which gives a succession of beats, and those beats form a source of sound. Now, with this source of sound, and his beautiful scientific apparatus or detector, he started upon one of Sir Wm. Thomson's discoveries, viz., that wires alter their electrical condition when they are placed under strain. He took a piece of wire, applied weight to it, connected the clock with it, and heard nothing. He was not disconcerted, he applied weight after weight till he reached the breaking strain of the wire, and at the moment when the wire broke, he heard a rush or sound which he thought was an indication of what he was searching for, so he took the two ends of his wire and laid them together, placed his source of sound above them, and to his intense delight heard—what imagination perhaps assisted him in believing to be—a tick. He thought he was on the right track, and he then manufactured with a flat piece of brass for a lever, a pin for an axle, sealing wax for cement, and black wax for solder, and the uncovered bonnet wire for binding, a little apparatus which enabled him to apply constant pressure to the thing he was experimenting upon; in fact, by this means, he was enabled to produce what electricians call a "bad joint." To his intense delight he found that with this bad joint he was able to obtain sonorous effects. But this contrivance, simple as it appears, was a great deal too elaborate and complicated for his purpose, so he took two little nails—the little bright nails so much used in France—laid them side by side, not touching each other, and bringing the ends of the wire in contact with them, and laying between, or across them, a third and similar nail, he was able to reproduce, almost perfectly, the sound of the clock, and more than that he began to get indications of the sound or tone of the voice. He then used chains; he took my gold chain and put it beneath his little compressor, and with that we were able to speak with great ease. From that he tried filings, and found with matter in a finely divided state, that he was able to reproduce all effects of sound. At last he made little glass tubes about two inches long, filling them with white bronze powder which artists use, which is a mixture of zinc and tin, and he was able to exactly reproduce the tones of the voice. But in his experiments with carbon he was able to make what may be called quite an independent discovery. The carbon he experimented with was the common carbon used by artists in sketching their drawings, and this carbon he found to be a non-conductor of electricity. The idea struck him that this non-conductor of electricity might be made a conductor, and by various processes he at last arrived at a plan of boiling or heating this carbon in quicksilver. Carbon so heated in an atmosphere of quicksilver itself becomes permeated through and through with quicksilver, and by that means we get the mercury subdivided into an infinitely fine state. Probably mercury in this state as closely approaches the molecular as anything can do.

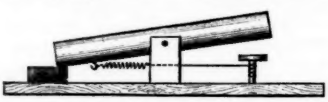


FIG. 1.

There is no apparent indication of mercury under the microscope, and yet we know that the carbon has been mercurised, because it is converted from a complete insulator into a conductor, and it has a metallic ring when it falls. Now then, having by these processes arrived at a substance which is remarkably sensitive to all the variations of the sound of the human voice, his next task was to construct these things into such a form as to make them telephonic transmitters. For that purpose he brought to his aid a very cheap kind of apparatus, a halfpenny money-box; inside this he placed his carbon transmitter, and as this discovery is not fenced in by fear of the patent or any other law, I am quite sure you will be glad to know how to make a Hughes transmitter. First he takes a piece of quarter-inch board about two inches long and one inch broad, and he raises upon that two thin brass bearings with a hole worked through by means of a pin for the support of the axis. He then takes a piece of carbon which has been mercurized about two inches long, which has a pin cemented to it near its centre, and which acts as an axis, and makes it into a lever. On the board he places a small piece of carbon, similarly treated, and upon this rests another similar sized piece of carbon, the two being connected by a piece of paper.

This is the end of one wire, and that the end of another wire; and on this diagram the arrangements of the circuit

are shown. S represents the source of sound, which I have shown on the black board. B represents the battery, and T the telephone. Now the battery is another remarkable speci-

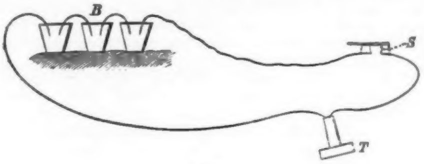


FIG. 2.

men of scientific manufacture. Three little glass tumblers are taken; at the bottom of each a coil of copper wire is coiled spirally. The copper wire is covered with a little sulphate of copper. The tumbler is then filled with moistened clay, and upon the top of the clay is placed a piece of scrap zinc. The three cells are placed in a cigar-box. S is what is called the box-transmitter. The tube-transmitter is shown on this diagram.

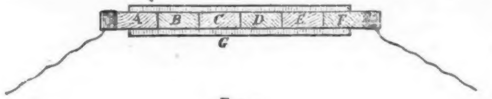


FIG. 3.

A is a glass tube about two inches long, and one quarter inch in diameter, inside which several pieces of mercurised carbon are inserted, touching each other with a pressure regulated by a screw fixed to each end. In that drawing there are six pieces of carbon, acting in this little transmitter; here (pointing) there are seven or eight, but that makes little difference, and the size of the carbon appears to be of little consequence. He has produced effects with carbon not larger than a pin's head. We shall show this by and by, but rather than disturb the order of these researches, I think it advisable in the next place to show you how this principle has been carried a little farther to produce what he calls the "microphone." This apparatus, drawn upon the board, is extremely sensitive. It will give evidence of nearly every sound; but in the microphone itself, which I have here, this extreme sensitiveness is carried to a still further extent. In point of fact, this is a microphone; but in this particular instance the pressure bearing between the two carbons is regulated by a spring fixed in this way, and it is so regulated that the transmitter is independent of any position in which it may be held. It is free to be moved in any direction in consequence of the pressure of the spring, but in one form of instrument this spring is dispensed with, and the pressure between the carbons is reduced to its greatest sensitiveness by making the two arms of this lever as short as possible. In the first machine he used, a piece of carbon was fixed on the top of an upright board, and a smaller piece was fixed down below. A cup-shaped hole was made in the upper piece of carbon, and a similar one in the bottom piece. Resting in these holes was a lozenge-shaped piece of carbon; and this lozenge-shaped piece of carbon rests with the greatest nicety upon its lower support, and is just in that position of equilibrium that the slightest atmospheric disturbance produces the effects which we are now about to show you. I think it desirable to tell you that you must not to-night expect distinct articulation. We have made a violent effort to make these experiments evident to you all. (Illustrations were given of speaking, singing, &c., &c.)

Now, the effects you have just heard have been produced by a transmitter similar to that drawn on the board. We will now repeat the effects with the machine on the table; and in order that you may judge of the effect—for Prof. Hughes desires that you should see there is no deception—we will connect this up, and use his old friend the clock to make its ticks, if it will, evident over the whole room. One of the greatest effects which this instrument produces is to render evident the tramp of a fly; and we have some nice little captives with which we will demonstrate that effect at the close of the meeting. (Illustration with clock.) To show that that is not due to the clock itself, Prof. Hughes will lift up the clock, when all traces of sound will have disappeared, and on putting it down again the sounds will be produced; so that the sound you hear is the sound of that clock which has been magnified. (This was so.)

Now, we have here a common quill pen, and Prof. Hughes will do as they do on the stage, pretend to write a letter; and I have no doubt if you listen attentively you will hear the scratching of his pen. (Illustrated.)

There are some peculiarities in this apparatus that are very striking. In the first place, though the sounds produced are very great, they do not interfere with each other. If you have a friend at the other end speaking to you, you can hear his voice distinctly working through your voice; and the result is you get a duplex action. Two or three persons can talk to each other without impediment or confusion.

Yet another point is, that the articulation is absolutely perfect. One of the great difficulties, both in the telephone and the phonograph, is getting the sibilant sounds reproduced—such as "s," and "c," and "sh," &c., which are produced by such extremely minute variations of the sonorous vibrations that they are lost in those instruments. Thus, if through the telephone you ask a person to "waltz," it will come out "walk," and names like my own, with the sound of "s" in it, would come out "Pree," not "Preece." In this transmitter one of its chief peculiarities is the fact that all sounds are faithfully reproduced; and it tends very much to upset the notion—Helmholtz's theory—that vowel sounds and other sounds are due to the superposition of waves upon waves of tones and over-tones. This apparatus shows almost unquestionably all these different properties; all these effects of intonation are due to differences in the form of the curve sent. Another peculiarity is this. I have told you that all in this room, every one's body while I am speaking, is alive with sound. If you take this transmitter and place it in front of your mouth, or put it on your forehead, or on the top of your head, or put it into your pocket, or upon your breast, it will still transmit sounds to distant places. Put it in a room, it does not matter where, it will reproduce the sounds. Put it anywhere in a drawing-room where there is a piano, you will hear the sounds of the piano faithfully reproduced. It is as you see a marvellously rough affair. You may throw it up, kick it about, or do what you like with it, it will always act. Here is the identical box that Prof. Hughes made two or three months ago. It has never been touched, it has been always at work, and never needs repair.

These are some of the peculiarities of his instrument, and I daresay some of you would like to know a little about its theory. We have here two points in contact, and those two points in contact complete an electrical circuit. The electric current that flows through that circuit depends for its strength entirely upon the obstacles or resistance in that circuit to the flow of the current; an alteration in any shape or form in the resistance of that circuit will result in the increase or decrease of the strength of the current flowing, and upon this diagram I have made a rough attempt to give you an idea of what occurs.

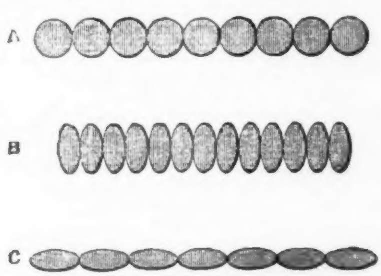


FIG. 4.

You must not conceive these round balls are molecules themselves, they are merely meant to represent the sphere of action of each molecule. In a normal state the molecules rest against each other, as shown by the upper line. When from any cause pressure is increased, they are contracted, as shown in the second line; when from any cause the pressure is decreased they expand, in the form shown on the other line. While I speak to you, the air of this room is thrown into vibration, the mass of air being subdivided into molecules in compression and molecules in extension. In a long wire these successions of compressions and extensions compensate each other; but when we break up a body into infinitely small parts, when we make less contact between two bodies, as shown there, and isolate the

portion of the sonorous wave in compression from that in extension, the result is that we have a variation in the resistance of the line. Now this variation in resistance depends upon the compression and dilatation of the molecules. They depend upon the tone of the voice, and the result is the resistance of the current varies with its variation of pressure, and at the distant end we have currents varying exactly as the voice varies, and reproducing on the telephone all the effects which we have seen. Hence follows the action of the microphone, and the action of the transmitter is one which depends upon the variation produced in bodies by the sonorous vibrations of the voice. As I am now speaking at that telephone, all the molecules of that transmitter are thrown into this elaborate series of compressions and dilatations. The current is varied; it goes to the room below, and is reproduced upon the telephone, as we have heard. Hence the effect is due to the difference of pressure, as is proved by using atmospheric pressure, and applying heat; and any large increase of pressure results in sound being reproduced.

No one has ever been nearer a great discovery than Mr. Edison. His telephone is based on the variation of resistance due to pressure. He used carbon and finely divided matter, but he worked on the idea that the difference in pressure was produced by the vibrations of a diaphragm. Had he thrown away his diaphragm he would have forestalled Prof. Hughes in this respect, and found that the sonorous vibrations themselves produced this difference of pressure. The great secret of Prof. Hughes's discovery is that sonorous vibrations and electrical waves are to a certain extent synonymous.

Now as to the uses to which this instrument is capable of being applied. It has been applied to surgical purposes in the form of the stethoscope. Though it does not show very markedly the beats of the heart, because they are more mechanical thumps than sonorous vibrations, yet it will show the injection and ejection of air in the lungs, and for many other surgical purposes it must become a valuable instrument. It admits us to some of the mysteries of insect life, and by its means we can hear sounds emitted by insects which have never been heard before. Going further it has enabled the deaf to hear; deaf persons who never heard a telephone before have been able to hear distinctly. It has enabled us to hear the physical operation which goes on in the process of crystallization of bodies and other things which before were wholly inaudible; and in fact it is impossible to say to what uses it may not be put.

It is rather remarkable that in an excellent paper read before the American Electrical Society, the author, Mr. Pope, makes these curious remarks:—

"The most striking results are to be looked for in the direction first pointed out by Mr. Gray, for the reason that if an effectual method of controlling the resistance of the circuit by means of atmospheric vibrations can be discovered, the source of power, which in this case is the battery, may be augmented to any required extent. It is not to be denied that the problem thus presented is one of exceeding mechanical difficulty, but there is no reason to suppose that it may not be successfully solved. It is to the development of this variety of the speaking telephone rather than to that of the magneto instrument that inventors will find it most advantageous to turn their attention, for I hazard little in saying that the latter has already reached such a surprising degree of efficiency, as to leave comparatively little more to be done within the necessary limitations which have been pointed out."

Mr. Pope throws out what has been done with the exception of the supposed mechanical difficulty, and that has been got over by a halfpenny money-box.

Now one very pleasing and gratifying circumstance attaches to this discovery of Prof. Hughes: he has thrown it open to the world, and by that means he has no doubt checked that species of immorality—I don't know what else to call it—connected with the infringement of the patent law, as regards the telephone. He allows us all to manufacture microphones for ourselves, but even he has been subjected to rather a peculiar incident. One impulsive and active gentleman who was present at the Royal Society the other night when Prof. Hughes first described his invention, went home and made himself a microphone, wrote a description of it and sent it off post haste to Paris. A short time afterwards Prof. Hughes himself with great care prepared a paper to be read before the French Academy, but to his great surprise he found that he had been forestalled, a description of his instrument had already appeared in the Paris prints from the gentleman in question.

There are lessons to be learnt from this discovery, and the principal lesson is—we can all of us with the means at our disposal cross-question nature and find out her secrets, and there are many secrets which yet remain to be divulged. We learn the wonderful connection which exists between all the physical forces: heat, and light, and electricity, and magnetism, are all co-related, and it has come to this, that what boys have said in joke has come to pass in earnest. We have been able to convert electricity into light, and light into electricity. We are now able to convert electricity into sound, and sound into electricity, and thus we are enabled to see the thunder and to hear the lightning.

THE SCIENTIFIC AIMS AND ACHIEVEMENTS OF CHEMISTRY[1]

MORE than a generation has passed away since my predecessor in the chair of Chemistry, Prof. Bischof, who was so full of merit in the domain of chemical geology, held the high office which the friendly confidence of my colleagues has entrusted to me for the ensuing university year. Since that time chemistry has undergone important changes, and its position upon the German high schools has also become an essentially different one.

At that time a general discouragement had taken root amongst the most eminent chemists. It was believed that all speculation had to be dismissed from the field of chemistry, and particularly that all atomistic considerations had to be discontinued, because whole categories of facts could not be made to agree either amongst each other, nor with the general theoretical views of that time.

At our high schools at that time chemistry was only taught from the chair; very often by teachers who were essentially appointed for other subjects. At most of the universities the students could be admitted to practical work only by favour of the teachers, and even Liebig's laboratory at Giessen, the first of all educational laboratories, only just then received its interior arrangement.

How different now! Well aware of its task and its aims, scientific chemistry, in close connection with physics, advances slowly, it is true, but with self-reliance and a certain confidence.

Each university has its special chair of chemistry, many indeed have several. Richly furnished laboratories, and very often luxurious edifices, are at the disposal of chemical students in nearly all German universities, and the chemical lectures are the best frequented ones almost everywhere.

All this and also the circumstance that it is just a chemist who is able to day, as a representative of the entire university, to speak to the entire university from this place, proves, doubtless, that our science is now generally recognised to the extent it merits. But as on many sides it is over-estimated, it is yet more frequent, on the other hand, that its scientific right of existence is doubted. While outsiders who may occasionally have seen a chemical experiment, or may have heard of the grand applications of chemistry to practice, declare chemistry to be the finest science of all, although they may not be able to form an idea concerning its scientific aims, other one-sided representatives of so-called humanistic studies, who also mix up the applications of chemistry with its scientific task, tend towards the unjustifiable view that chemistry ought really to be taught only at polytechnic schools, but not at the "universitas litterarum."

The propagation of such erroneous conceptions renders it the duty of the chemist to appear as the defender of the science he represents, and it will doubtless be considered fully justified if to-day I try to explain to you the scientific position of chemistry and its participation in the great progress of universal science.

Chemistry has often been designated as the sister of physics, and both subjects are in reality so nearly related, their domains are so contiguous, that the layman cannot understand the difference, and that even the scientific man can hardly fix the limits.

Chemistry and physics together form that group which may be designated as *general natural science*, inasmuch as the occurrence of their materials of study is unessential, and the laws recognised by them are valid everywhere. Astronomy, geography, geology, botany, and zoology (the latter including those more special subjects treating of man, and which form the scientific part of medicine), all these, which ought to be comprised under the

[1] Address delivered on assuming the Rectorate of the Rhenish Friedrich-Wilhelms University of Bonn, October 18, 1877, by Prof. Aug. Kekulé.

name of *special natural science*, are tied to certain circles of objects of study, and the truths they have established are valid only for these circles. Even that generalisation of the so-called organic natural sciences, which is designated as biology, cannot very well be called a general natural science, because if indeed anywhere else than upon the earth there exists something similar to what we call life here, there is yet but little probability that the laws of terrestrial life may be applied to that life of other worlds.

The common task of *general natural science*—of physics and chemistry, therefore—is the investigation of matter, of its properties, its changes, and of the laws of these changes; and the laws recognised by them must be applicable wherever there is matter at all.

Now with regard to the *difference* between *physics* and *chemistry*, it strikes the superficial observer that modern physics treats, in a more general manner, of the properties and changes of properties of bodies, and in doing so that it contemplates the separate bodies only as the bearers of the properties; while chemistry studies the separate bodies differing with regard to their material, and that it touches their properties only inasmuch as they appear necessary for distinguishing the bodies. One might be inclined to found a definition of the two disciplines upon these differences. But when we enter more deeply into the subject we shall see that the essential differences must be looked for elsewhere.

Of all conceptions which the human mind could form regarding the essence of matter, only the hypothesis of *discrete* mass-particles, the atomistic hypothesis therefore, has led to an intelligible explanation of facts. Even if nobody who has followed the scientific discussions of the latest time, can deny that the tendency of natural scientific reflection just now again lies in the direction of reducing the differences of materials to dynamic causes, yet we must certainly own that *at present* the observed facts can be deduced as necessary consequences from the atomistic theory only. On this point physicists and chemists are no doubt agreed. And if even modern representatives of speculative philosophy concur in the view that all natural knowledge leads to the mechanics of atoms in the last instance, then we may doubtless use the atomistic theory *preliminarily* as the basis of further reflections in the domain of natural science and, *for the present at least*, found upon it the definition of the separate branches of natural science, be it only in order to render a clearer account to ourselves of their tenour and of the limits of their domains. Now the sum total of all knowledge obtained with regard to matter has led to the following *maxims of the atomistic theory*.

We must imagine that matter consists of small particles, uniform in their material and not further divisible, not even by chemical processes,—of *atoms*. These atoms accumulate in consequence of forces inherent in them or acting upon them, and thus produce systems of atoms, or *molecules*. In the gaseous state these molecules move about in space as isolated beings, in the other aggregate states an attraction of molecules also becomes apparent, and thus the *masses* originate which are able to act upon our senses directly.

If this conception of the essence of matter is taken as a basis then we may define chemistry as the *science of atoms*, and physics as the *science of molecules*, and it lies near then to look upon that part of modern physics which treats of *masses* as a separate discipline, and to reserve for it the name of *mechanics*. Thus mechanics appears as the fundamental science of physics and chemistry, inasmuch as both are obliged to treat their molecules or atoms respectively as masses in certain considerations, and particularly in calculations. Mechanics, physics, and chemistry, however, are the bases of all special natural sciences, because it is evident that all changes, no matter whether they occur, in the great cosmos, or in the microcosmos of the vegetable or animal body, can but be of a mechanical, physical, or chemical nature.

Now from the fact that chemistry has to do with the study of atoms, of the building stones, therefore, of which the molecules are constructed which physics treats as a whole, it results directly that the theoretical investigations of chemistry offer more difficulties than those of physics, and that theoretical chemistry can progress in certain directions only after theoretical-physical knowledge has sufficiently advanced. The comparatively low state of theoretical chemistry thus seems not only pardonable but natural, and it becomes clear why for the present theoretical-chemical investigation has principally turned its attention to those questions which are more or less independent of physics. Thus we understand why *chemical dynamics* is as yet an almost uncultivated field upon which the materials, which are heaped up in immense profusion could, up to the present, not find a theoretical treatment, while on the domain of *chemical statics* ripe, or at least partly-developed fruits, were reaped in plentiful quantity.

It will not be difficult to show that chemistry and chemists have, in this direction, materially contributed towards the progress of the general doctrine of atoms, therefore towards the progress of our knowledge of the nature of matter.

Since the (as far as we know) first foundations of the scientific observation of nature were laid by Democritus, the most elementary maxims of the theory of matter have remained the same. "From nothing nothing can come; nothing that is can be annihilated; all change is only combination or separation of particles." But the atomistic theory of antiquity was more a precursor of the views which we now designate in physics as the molecular theory; it contained, even in its further development, no fundamental thought of a specially chemical theory.

The first fundamental maxim of scientific chemistry was pronounced towards the end of the seventeenth century by the chemist Boyle, who was first to define the conception of the *chemical element as that which is not further divisible into materially different parts*. It will not matter whether many or perhaps all the bodies which we now consider to be chemical elments may be found to be further divisible in the progress of knowledge—although there is at present no real indication for this—the idea of the chemical element will always remain unaltered.

With this idea of the chemical element that old conception of the indestructibility of matter was then connected, and thus the further fundamental maxim of chemistry originated, of the invariability of elements, which has not further been questioned since Lavoisier's celebrated experiments on the often-pretended change of water into earth, and which finds its confirmation in all chemical facts.

From these views the *chemical atomic theory* arose at the beginning of the nineteenth century, and the English chemist Dalton is with right regarded as its founder. While, after Democritus, the difference of all things is caused by the difference of their atoms in number, size, shape, and order, a *qualitative* difference of atoms, however, does not exist, Dalton first in a definite manner supposed the existence of *qualitatively different* elementary atoms. He was first to ascribe to these qualitatively different atoms *certain weights* which are *characteristic* for the various elements; he first showed that these relative *atomic weights* may be determined by chemical study.

As the conception of the chemical element so will also the conception of the chemical atom, as *that quantity of elementary matter which is not further divisible by chemical processes*, remain for ever. For chemistry, the question whether the chemical atoms are originally units (*einheitliche*) and absolutely indivisible beings, is of no importance. Let the proof be given that the chemical atoms are formed of particles of a finer order, or let the theory of revolving rings founded by Thomson, or some other similar conception which understands atoms to result from continuous matter, be proved in the progress of knowledge, the conception of chemical atoms will not be altered or annihilated. The chemist will always welcome an explanation of his units, because chemistry requires atoms only as a starting point, not as an end.

Dalton's atomic theory from the very first suffered from a certain imperfection which consisted in its speaking of the atoms of compound bodies as well as of those of elementary ones and thus did not distinguish the ideas of atom and molecule. For the first period, during which the foundations of chemical science had to be completed, no essential harm arose from this want of clearness, but later on, when the structure was to be developed further, it caused considerable confusion.

It is true that already in 1811 Amadeo Avogadro pronounced the maxim that gaseous bodies contain an equal number of molecules in equal spaces, and that even the molecules of elementary substances consist of several atoms, and that in 1814 the French physicist Ampère arrived at the same conclusions; but this idea, which was so fertile in the future, hardly attracted any notice at first. In its application it led to contradictions which seemed insolvable at that time, and it was therefore abandoned, although the great chemist Dumas had taken it for some time as the base of his considerations. More than that, it was forgotten until forty years later the Italian chemist Cannizzaro recalled to the memory of his colleagues the merits of his countryman.

In the meantime chemists first, and later on physicists as well,

had arrived at precisely the same conceptions, starting from new and perfectly independent points of view.

The chemists, with Laurent and Gerhardt as leaders, were led by *purely chemical* considerations and essentially by reasons connected with systematics, to distinguish clearly between the ideas of atom and molecule, and to find methods which, *in the perfection at which they have now arrived*, render possible the determination of the relative weights of the atoms and molecules, and even of the *absolute* number of atoms in the molecules, for all more perfectly examined substances, by the discussion of purely chemical facts. Amongst others they arrived at the result that the molecule even of elements consists of two atoms as a rule.

In physics, however, the *mechanical heat theory* caused a probability bordering upon certainty to be ascribed to the fundamental thought of Avogadro's hypothesis; and when our celebrated colleague, Clausius, in the course of his classical investigations, had arrived at the conception that even in elements the molecules consisted of several atoms, then he could express his satisfaction regarding the fact that chemists *before him*, on totally different ways, had already arrived at the same results.

After, in this manner, Avogadro's hypothesis on the nature of gases had obtained general recognition, and the relative weights of the gas particles could thus be deduced from the specific gravities of gases; after, on the other hand, we had learned to determine the relative weights of the chemical molecules by chemical considerations, then it appeared that both values were identical, and thus we arrived at the conception, which anyhow was probable on account of its simplicity, but which was not a necessary one previously, that the *gas particles* and the *chemical molecules* are identical, that heat therefore, is able to subdivide matter down to the chemical molecules.

The *chemical* part of the atomic theory was essentially extended some twenty years ago by that hypothesis, made by chemists, which has been designated as the theory of the *chemical quantivalence* of atoms. In its fundamental thought this hypothesis only says that *besides* the characteristic atomic weight which is the cause that the elements combine in certain *proportions of weight*, the *atoms* still possess a further fundamental property, which causes them to combine exactly *in those numbers* in which they do. As we could not, at first, arrive at a clear conception of this fundamental property, we simply ascribed a certain number of chemical attraction units to the materially different atoms, and accordingly called them uni-, bi-, tri-, or quadrivalent.

Now this hypothesis of the chemical quantivalence of elementary atoms of course still offers many dark points, but yet it has led to the recognition of a law which, not only for chemistry, but for the entire atomic theory is of fundamental importance, and which chemists call the law of the *connection of atoms* (*Verkettung der Atome*). *The separate atoms of a molecule are not connected all with all, or all with one, but, on the contrary, each one is connected only with one or with a few neighbouring atoms, just as in a chain link is connected with link.*

At the same time it is evident that the atoms within the molecules must be in constant *motion*, and if, indeed, nothing certain is known respecting the nature of this motion, yet it results from this very law of the connection that the intramolecular atomic motion must be of such a nature that the separate atoms move about certain positions of equilibrium without ever leaving them, as long as chemically the molecules continue to exist. The motion of atoms, therefore, is certainly similar to that of the molecules in the solid state, and thus it may be said that the *molecules of existing substances* are *solid aggregations of atoms.* A state of motion similar to that which the molecules of liquid bodies possess, occurs only with chemical changes, by which molecules of different atomic structure are formed, and then evidently only in a transitory manner and only for single atoms. A state of this kind doubtless plays an important part not only in fermentation phenomena, but also in the chemical processes occurring in living organisms. The nature of the motion of atoms is, as I said before, unknown at present. Perhaps it may be imagined as an oscillatory one in such a way *that the number of oscillations executed in the unit of time* exactly represents the chemical value, and that atoms engaged in functional oscillation, and perhaps striking against each other, appear in chemical combination. Then the chemical quantivalence of atoms would have to be considered as a *constant* one with even greater probability than hitherto. Anyhow one might imagine that polyvalent atoms, at temperatures which, for the substances in question, might be called *ultra-hot*, do not meet with another atom during one or even more oscillation phases, while adding a part of their motion-energy to the molecular motion; a conception which would correspond to the present conception of unsaturated affinities. We would have to think it probable, further, that upon raising the temperature still more, this intermediary state of partial dissociation would be followed by one of total dissociation, during which isolated atoms move in space, just as has already been proved for mercury vapour at temperatures of easy access.

The law of the connection of atoms based upon the hypothesis of chemical quantivalence, at present accounts only for the *chemical serial connection* (*Aneinanderreihung*) of atoms, without explaining their *position in space* and the *form of molecules* caused by it. But even now, from investigations on molecular volumes it results that the nature of the connection of atoms influences the *mean distances of atoms.*

The circumstance that with isomeric substances the boiling-point of that modification is the highest for which the law of the connection of atoms supposes a chain running in a straight line, while volatility increases the more ramifications the chain shows, the more compressed, therefore, the molecule appears from a chemical point of view; together with the maxim, probable in itself, that the position of the point of gravity and the moment of inertia of the rotating molecule must be of influence upon volatility, seem to indicate that the views on the chemical connection of atoms at the same time give us some notion on *their mean position in space.* The calculations made by Emil Meyer, of the molecular diameters, molecular transverse sections, and molecular volumes, also seem to support this view. Thus the probability of the hypothesis pronounced by Le Bel, and worked out further by Van't Hoff, of the *unsymmetrical carbon*, according to which the four affinity bonds of the carbon atoms, which had already been represented tetrahedrically, are imagined to exist *in space* in a tetrahedrical position, is increased. An hypothesis which may perhaps not merit the unconditional praise which Wislicenus has bestowed upon it, but which certainly much less deserves the bitter derision which Kolbe was inclined to throw upon it.

The hypothesis of chemical quantivalence further leads to the supposition that also a considerably large number of single molecules may, through polyvalent atoms, combine to *net-like*, and if we like to say so, *sponge-like masses*, in order thus to produce those *molecular masses* which resist diffusion, and which, according to Graham's proposition, are called *colloidal* ones. The same hypothesis in the most natural manner leads to the view, already pronounced by our genial colleague, Pflüger, that such an accumulation of molecules may go further yet, and may thus form the *elements of the form* of living organisms. Of these *mass-molecules* we may perhaps suppose further that they, through the constant change of position of polyvalent atoms, show a constant change in the connected single molecules, so that the whole—and of course under generation of electricity—is in a sort of living state, particularly since, through the same change of position, adjacent molecules are drawn into the circle of combination and newly-formed ones are expelled. To follow such speculations any further at present would, however, be equivalent to leaving the basis of facts rather too far behind us.

Really fertile hypotheses on the *nature of that force*, which brings about the combination of atoms, have not been made up to the present. The *electro chemical theory*, so ingeniously worked out by the great Berzelius, of which, during whole decades, it was believed that it would lead to a satisfactory explanation of chemical facts and to their combination with physical phenomena has proved completely insufficient. In all probability in a future period of the development of science it will again be taken up, and will then, in a modernised form, bear its fruits.

In any case besides the chemical quantivalence, which decides the *number* of combining atoms, the *specific intensity* with which this combination takes place, must also be considered. Here we must suppose that the atoms combined in a molecule, and therefore saturated with regard to their quantivalence, do not only exercise an attraction upon each other but also upon the atoms of neighbouring molecules, and that thus a *molecular attraction* results, which is caused by the attraction of the separate atoms and therefore depends on their quality. Only in this way we can explain the process of chemical decomposition and the existence of that infinite number of more complicated things which are supposed to be *molecular additions* or molecules of a *higher order.* Unquestionably the same cause plays a part in so-called *mass-effects* and in *catalytic* decompositions. The formation of solutions must also be ascribed to it, which hitherto were considered as chemical combinations in varying proportions, but

which are now more appropriately called *molecular mixtures*. The same fundamental cause further gives rise to the phenomena of cohesion, adhesion, and capillary attraction, and it seems therefore as if the supposition of special molecular forces is in no way necessary any longer.

Now as the attraction of atoms depends on their *quality*, it is also clear that the molecular attraction caused by such atomic attraction must, under favourable conditions, produce an *orientation* of all molecules combining with one another, and must thus lead to bodies of a regular molecular structure, therefore to *crystals*.

Lastly, the question whether the properties of atoms are dependent on their *weight* has much occupied the chemists of modern times. Positive results which could be rendered clear in a few words have not yet been obtained, but it seems, according to the observations made by Lothar Meyer and Mendelejeff, as if not only the *chemical* properties and specially the chemical quantivalence of atoms and the intensity of their mutual combination, but also the *physical* properties, which at present are still treated as *constants* for materially different objects, were a function and indeed a *periodic function* of the atomic weight. The mathematical form of this function is no doubt of a peculiar nature, but one thing seems certain, viz., *that the numerical value of the atomic weight is the variable by which the substantial nature and all properties dependent on this are determined.*

Thus there again seems hope that it will be possible to reduce *all* properties of matter, including gravity, to *one* and the same force.

The right of introducing all such speculations into the domain of exact science, has been questioned very much. It is generally conceded, indeed, that the setting-up of hypotheses on the domain accessible to exact investigation, as a method of investigation, is useful, inasmuch as it often may accelerate the progress of exact knowledge. But it is at the same time often believed that speculations beyond a certain limit are not admissible. The scientific value of all atomistic considerations particularly has ever, and also in the most recent time, been very much doubted. It has been pretended specially that the supposition of atoms did not explain any properties of bodies which had not first been ascribed to the atoms themselves.

We must own that such remarks contain many truths, but just for that reason it seems necessary that we examine the limit of their correctness.

It is generally acknowledged that the results of exact observation have the value of facts, therefore possess that degree of certainty which human knowledge can attain at all. It is further not contested that to all those laws which, independent of hypotheses on the nature of matter, are deduced from facts, nearly the same certainty must be ascribed as to facts themselves. It is just as incontestable, however, that the human mind in the positive understanding of facts does not find complete satisfaction, and that therefore natural sciences have to follow a yet further and higher aim, *that of the knowledge of the essence of matter and of the original connection of all phenomena.*

But the essence of matter is not accessible to any direct investigation. We can only draw conclusions regarding it from the phenomena which are accessible to our observation. And thus it is evident that there is a certain limit which, moreover, is influenced by the state of our knowledge at any given time, beyond which positive investigation loses ground and where the path is only open for speculation.

If, therefore, the single investigator, following the inclinations of his nature, rests satisfied with positive investigations and renounces all speculations, it is yet clear that to science as such this is not permitted.

By way of hypothesis, based upon what is known as facts, ideas must be formed on the nature of matter; the consequences of these ideas must be developed logically, and, if necessary, by the aid of calculation, and the results of these theories must be compared with the phenomena accessible to observation.

Of course, the *complete* truth will never be reached in this way, or there will, at least, never exist complete certainty that our conceptions are really identical with truth. But that conception which is simplest in itself, and which in the simplest manner accounts for the greatest number of phenomena, and finally for all, will have to be considered not only as the best and most probable one, but we shall have to designate it as relatively, and we may say, humanly, true.

By this the scientific right of existence of speculative investigation is no doubt proved, also for the so-called exact sciences, because beyond a certain limit these indeed cease to be exact.

Simultaneously, however, the scientific value of the present atomic theory is also proved, because it has not been contested that, even in its present and still extremely incomplete form, it accounts satisfactorily for an uncommonly large number of facts, better than any other conception.

It will certainly require further extension, and also a deeper fundamental structure; but at present there is very little probability that it will be completely superseded by essentially different conceptions.

There are other reproaches which have been made to chemistry specially, and still more to chemists, now and since the time of Lord Bacon; and even chemists cannot deny that they were not altogether undeserved.

It has been said that chemistry wilfully makes innumerable single hypotheses which are neither in connection with one another nor with the whole; that the value of hypotheses is over-rated by her disciples, far too great certainty being ascribed even to such as are only little justifiable, and that they are treated as if they had been actually proved; and finally, that her hypotheses are always gradually raised to articles of faith, and that everybody who sins against such dogmas is prosecuted as a heretic.

Recent times have also, in this direction, brought about a considerable improvement. The justification and the value of hypotheses are now recognised in chemistry, but at the same time the true value of hypotheses is also understood by chemists.

In chemistry also, as in all domains of science, blind faith in authorities has been crushed, and by this alone the danger of dogmatising is lessened. And should perhaps any one, who holds antiquated views, try to attach his dogma upon progressing science as a restraint, he will always find the striving young generation, the representatives of the future, ready to remove unjustified impediments. If others, in the fiery zeal of youth, should be inclined to look upon daring flights of fancy as scientific hypotheses and to give them out as such, then those who are more moderate by themselves or by the riper experience of age, will always feel it a duty to step in as regulators.

The school of independent, and at the same time quiet thinkers, is now so numerously represented also among chemists, that a constant development of the science may be confidently expected, and an overgrowth of weeds need no longer be feared. Also in chemistry we are now well aware of the continuity of human mental work; the present generation no longer looks with despising contempt upon the work of their predecessors; far from thinking themselves infallible, they know that at any time it remains to the future to continue the work of preceding generations.

ON THE CAUSES OF THE ASCENT OF SAP IN TREES[1]

THE question as to what forces cause water to rise to such a remarkable height (frequently) in trees has had very various answers given to it. But these have mostly failed to account adequately for the phenomenon.

Capillary action is perhaps the oldest cause adduced. The view was long popular that water rose in trees like oil in a wick, the connected vessels of the wood forming capillary tubes. This view lost force when it was known that the wood of coniferæ was without vessels; and it did not explain the weakening or stoppage of the rise of sap produced by amputation of the roots, nor the presence of air in the columns of sap.

Shortly after Dutrochet's thorough study of diffusion, this phenomenon was called in to account for the rise of sap. One grave objection to such a theory is the rapidity of the ascent of sap (it has been carefully measured) as compared with the slowness of diffusion, which depends simply on molecular motion; another is the inevitable consumption of the osmotic force of tension. So that other problematical forces had to be called in.

When Jamin found that the imbibition of water through fine porous substances (*e.g.* blocks of gypsum) took place with great force, and that the air could thus be compressed to several atmospheres, an effect of this nature was affirmed to occur in living plants, the cell membrane being considered a porous substance. But in fact the natural saturated cell membrane has no air-filled pores, but only pores already filled with water, and even the hollow spaces, bounded by the cell membrane, are partly filled with water; besides, the fact that a branch, immediately after being cut off, loses in great measure the power of raising water, is against this theory.

[1] Abstract of a recent paper in *Der Naturforscher.*

A few years ago yet another theory was started, based on M. Quincke's discovery of the tendency of liquid films to expand rapidly upon wetable surfaces. The only advantage of this lay in accounting for the rapidity of the rise of sap; otherwise it was open to all the objections of the Jamin theory.

A theory has lately been propounded, and thoroughly worked out, by M. Joseph Böhm, which is characterised by good consistency, and offers perhaps a more satisfactory explanation of the phenomenon than any that have been referred to. It is based, like the osmotic theory, on the cellular structure of all sap-conducting plants, and it attributes an important *rôle* to the *elasticity of the cells*. "When the surface-cells of a plant," says M. Böhm, "have lost a portion of their water through evaporation, they are somewhat compressed by the air-pressure. Like elastic bladders, however, they tend to take their original form. This of course is only possible by their drawing in either air or water from without. Since, however, moist membranes are little penetrable by air, the cells draw from cells further in a portion of their liquid contents. These again borrow from their neighbours further down, which contain more water, and so on, either to the extreme root-cells or to those parts of the stem which are supplied with water from below through root-pressure."

To illustrate the action M. Böhm constructed an artificial cell-chain. A funnel closed by a bladder represented the evaporating leaf; to it were connected below several glass tubes about two ctm. wide, closed at one end with a bladder, and joined together in series by means of thick-walled caoutchouc-tubing. In consequence of the evaporation, the membrane which closes the funnel-mouth is bent inwards, and when it has reached a certain tension water is sucked into the funnel out of the next lower cell, which covers its loss in like manner. Manometers, connected with certain cells of the apparatus, indicate the amount of suction at different heights. To avoid fouling of the membranes carbolic acid was mixed with the distilled water in the cells. Since bladder membranes, with a not very great height of liquid column over them, admit passage of water by filtration, these artificial cell-chains (it is pointed out) must act much more imperfectly than the sap-conducting cells placed over one another in living plants, which cells, by reason of their narrow aperture, retain their liquid column by capillary attraction.

It is shown that this theory is in harmony with sundry phenomena which are contradictory of the imbibition theory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

It will be proposed to confer the degree of D.C.L. *honoris causa* at the ensuing Oxford commemoration, upon Dr. William Spottiswoode, M.A., of Balliol College, F.R.S.

The following awards for proficiency in Natural Science have been made at St. John's College, Cambridge:—Foundation Scholarships to F. J. Allen, Marr, Slater, C. M. Stuart; Exhibitions to Fleming, Hart; the Open Exhibition to C. H. O. Curtis, from the Royal School of Mines.

The plans for the new University edifices at Strasburg have just been completed. They provide for over 100 rooms to serve as auditoriums, museums, the inevitable German singing hall and fencing hall, &c., and will meet the needs of all sections of the university, with the exception of the medical faculty, which retains its old quarters, on account of the propinquity to the hospital. The attendance, which has fallen off during the past year, is now greater than ever before, the number of students for the present semester being 710.

SCIENTIFIC SERIALS

Journal de Physique, April.—In this number M. Vincent recommends chloride of methyl as a frigorific agent, and indicates an abundant source of it. He employs a cylindrical copper vessel having double walls, between which the liquid is admitted through a peculiar cock from an adjoining vessel. In the central part is put an uncongealable liquid such as alcohol. The outer wall is enveloped in cork. On opening the cock the chloride of methyl enters into ebullition, and the temperature of the alcohol bath sinks to $-23°$. By connecting with an air pump and making vacuum, a much lower temperature may be obtained. One pretty experiment with this apparatus is the crystallisation of mercury.—M. Gariel explains the new system of numbering glasses of spectacles, in which a unit called the *dioptrie* is used, this being the power of a convergent lens of 1m. focal distance. The number of dioptries for a particular lens is got by dividing 1m. by the focal distance reckoned in metres and decimal fractions of a metre, since the power varies in inverse ratio of the focal distance. Let N_D be the number of a lens reckoned in dioptries and f_m the focal distance in metres, then $N_D f_m = 1$m., which gives one of the quantities when the other is known.—M. Pellat contributes a mathematical paper on the specific heats of vapours, and the phonograph occupies some attention.

Memorie della Società degli Spettroscopisti Italiani, January, 1878.—Prof. Tacchini contributes a long paper on the appearance and constitution of the sun, based on the photographs of M. Jansen taken at Meudon; there is also another by the same author, giving the observations of the positions in which the magnesium and 1474 lines appeared on the limb of the sun in June, 1877. The appendix contains a paper by L. Gruber on the falling stars of the first part of last November.

February.—Notice of the death of Father Secchi, by the editor.—A paper by Prof. Rosetti on the temperature of the sun; a description of the thermopile and the necessary accessories, together with the results, is given at length.—A table showing the number of spots and protuberances, and the heights of the latter during the first half of the year 1877, and drawings of the chromosphere for the months of November and December made at Rome, by Prof. Tacchini.

March.—A note and table by Prof. Tacchini showing the position on the sun's limb when the magnesium and 1474 lines were seen during June, 1877. Also a summary of the positions of the same during the first half of the year 1877.

SOCIETIES AND ACADEMIES

LONDON

Royal Society.—"Note on the Specific Gravity of the Vapours of the Chlorides of Thallium and Lead," by Henry E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester.

Experimental difficulties of so serious a nature surround the attempt to ascertain the specific gravity of vapours at a high temperature that, in spite of the interest which attaches to this subject, but few additions have been made in our knowledge in this direction since the researches of Deville and Troost.

The present experiments, of which this notice contains the first results, have been made with the object of so simplifying the process as to render it easy to determine the specific gravity of the vapours of bodies possessing high boiling points with a degree of accuracy sufficient for the purpose of controlling their molecular weights.

The method consists in vaporising the substance under examination in long-necked glazed porcelain globes of known capacity placed in a muffle raised to bright redness. The temperature of the globe is ascertained by a calorimetric determination made with heavy platinum weights placed in the muffle, this determination being checked by the simultaneous insertion in the muffle of a second globe containing mercury.

The porcelain globes having a capacity of about 300 cub. centims., and containing from three to nine grams of substance, are closed by loosely-fitting stoppers of baked clay, and then gradually introduced in the muffle. After remaining there until no further escape of vapour is observed, and until the temperature has become constant, the globes are quickly withdrawn from the muffle and their contents removed and analysed, the temperature being in each case ascertained by the calorimetric method at the time of withdrawal of the globe. The following determinations of the specific gravity of mercury vapour serve to show the reliability of the method:—

	Temperature determined calorimetrically.	Specific gravity of mercury vapour.
	°	
Experiment I.	1019	6·92
,, II.	894	6·75
,, III.	815	6·91
,, IV.	972	5·77
,, V.	1047	7·05

the calculated specific gravity (Hg = 198·8) being 6·728.

Before determining the specific gravity of the vapour of thallium chloride it was ascertained that this compound does not

give off free chlorine when volatilised at a red-heat, and that the sublimate contains thallium and chlorine in the atomic ratio of equality.

In each experiment the total amount of thallium and of chlorine remaining in the globe was determined by analysis, and the specific gravity calculated from their sum.

	Temperature determined calorimetrically.	Specific gravity of the vapour of thallium chloride.
Experiment I.	859	8·15
,, II.	828	8·28
,, III.	1015	8·06
,, IV.	859	7·43
,, V.	1026	8·75
,, VI.	852	8·60
,, VII.	837	7·84

The specific gravity of thallium chloride vapour calculated upon the supposition that the molecular weight of the compound is 238·07, and its formula TlCl, is 8·49.

Four determinations of the specific gravity of mercury vapour made simultaneously with four of the above experiments gave as a mean the number 6·0 instead of 6·728.

The specific gravity of the vapour of lead chloride was made in a similar way, but the temperature required for complete volatilisation is much higher than that needed in the case of the last compound. The residue left in the globes was completely soluble in hot water, and contained lead and chlorine in the proportion of one atom of the former to 2·08 of the latter.

	Temperature determined calorimetrically.	Specific gravity of the vapour of lead chloride.
Experiment I.	1046°	9·12
,, II.	1089	9·72
,, III.	1077	9·51
,, IV.	1070	9·64

The specific gravity calculated from the formula $PbCl_2 = 277·14$ is 9·62.

I hope before long to be able to lay before the Society the results of specific gravity determinations of the vapours of other compound and elementary bodies, together with the whole of the experimental details.

Anthropological Institute, May 14.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Capt. Dillon exhibited a series of flint implements, collected in the neighbourhood of Ditchley, Oxfordshire, and a number of others, from the drift gravel of the sea valley near Clapton, were exhibited by Mr. Worthington G. Smith. The following papers were read by the author, Prof. Rolleston, M.P., F.R.S.—Description of a male skeleton found at Cissbury by Mr. J. Park Harrison. The paper was illustrated by a semidiagrammatic of the pit whence the skeleton had come; the principal parts of the skeleton itself, some bones of ox, goat, pig, and red deer, and finally, a large quantity of worked flints and some lumps of iron pyrites were upon the table. Much help had been received as to the preservation of the skeleton from Dr. Kelly, the Medical Officer of Health for the district. There was no doubt the skeleton had belonged to a man with a markedly dolichocephalic skull, the length-breadth index being 71, but not tapeinocephalic, the length-height index being 76; his stature had been something under 5 feet, either as calculated from the long bones or by simple measurement of the skeleton as laid out and increased by the addition of one inch for calcaneal and cranial integuments. The age had been something between 25 and 30, the absence of wear on the wisdom teeth being deceptive owing to the non-development of one of these teeth and the small size of another. The owner of the skeleton had suffered from infantile cerebral hemiplegia, the right humeous being half an inch longer, and the right radius $\frac{3}{10}''$ longer than the corresponding bone on the left side, whilst the femur were equal in length, and the right tibia only $\frac{2}{10}''$ longer than the left. This pathological condition, however, did not account for some very striking characters of the limb-bones, which were equally prominent on both sides of the body: these being the platyenemy of the tibiæ, the anterior convexity, and from side to side flattening of the humeri, and the curved upward end of the illux. Altogether the osteological peculiarities of the skeleton were as distinct evidences for its antiquity as its mode of burial.—On the excavation of three round barrows at Sigwell, near South Cadbury, in the parish of Compton, Somerset. These three round barrows belonged to the bronze age, no trace of iron, except such as had been accidently, and demonstrably so, introduced, being found in any of them. The interments in them had been in the way of cremation, and in one case the ashes had been gathered into a bark coffin and a bronze dagger placed with them. In one barrow no interment was found; in another the ashes occupied an area of only an inch in diameter; and in both cases the bones had been carefully picked out of the embers of the funeral pile and interred apart, though, in neither case, in an urn. Fragmentary pieces of coarse pottery, however, were found here and there throughout the mass of the barrows, and, though there were no flints to be found in the immediate neighbourhood, great abundance of chipped flints and some scrapers were found, and notably one very beautiful one by the Rev. J. A. Bennett, to whose association very much of the success of the exploration was due.

Physical Society, May 11.—Prof. W. G. Adams, president, in the chair.—The following candidate was elected a Member of the Society: Rev. P. Magnus, B.A., B.Sc.—Mr. J. Norman Lockyer, F.R.S., read a paper on some recent researches in solar chemistry, a report of which is deferred for the present.—Sir William Thomson, LL.D., F.R.S., described and exhibited the apparatus he has employed in recent researches on the influence of stress on magnetisation, a detailed account of which he has submitted to the Royal Society; he also, in part, described them at the Royal Institution on May 10, but attention was not then directed to the experimental details now brought before the Society. The rod or wire under examination was surrounded by two co-axial wire helices, the outer of which was connected with the battery, and the inner with a ballistic galvanometer, that is, one that acts with regard to electric impulses just as Robins' ballistic pendulum. It was some years ago discovered by Villari that a longitudinal pull augments the temporary induced magnetism of soft iron bars or wires when the magnetising force is less than a certain critical value, and diminishes it when the magnetising force exceeds that value; in either case the residual magnetism is augmented when the force is applied and diminished when it is removed. Sir W. Thomson has found the critical value for soft iron to be about twenty-four times the vertical component of the earth's magnetic force. It is therefore approximately 10 C.G.S. units. In the case of some bars of nickel and cobalt specially prepared for him by Mr. Wharton, of Philadelphia, he finds opposite effects. With the amounts of magnetising force used the effect of pull was to diminish magnetisation, but the amount of this effect was less with the highest magnetising forces than with a certain degree of magnetising force which was found to make it a maximum with probably or possibly a critical value. But this value had not been reached by the magnetising force hitherto applied. The next branch of the inquiry had reference to the transverse stress obtained by water pressure within a gun-barrel, and it was ascertained to have opposite effects to those found by Villari in the case of longitudinal pull. The critical point in soft iron for transverse pull is at about 25 C.G.S. units. Sir W. Thomson has been examining the effect of torsion on a wire that is at the same time exposed to longitudinal pull, confining himself in his first set of experiments to magnetisation under the sole influence of the vertical component of terrestrial magnetism. His results showed, with every amount of longitudinal pull, a diminution of magnetisation produced by torsion in either direction, thus extending a conclusion arrived at by Matteucci, Wertheim, and Wiedemann, regarding the effect of torsion unaccompanied by longitudinal stress. But it now appears that this effect of torsion is very remarkably diminished by a large pulling force nearly reaching the limits of elasticity. In conclusion, Sir W. Thomson called attention to a very different and extremely interesting effect of torsion discovered by Wiedemann —the development of longitudinal magnetisation in an iron wire by twisting it while a current of electricity is flowing along it. This effect, he pointed out, is just what would result from the æolotropic susceptibility for magnetisation due to the æolotropic stress produced in the outer portion of the wire by the torsion, supposing the tangential magnetising force to be less than a certain critical value intermediate between the Villari critical value and the more than twofold greater critical value which Sir W. Thomson has found for transverse magnetising force. But he pointed out that another cause was also positively or negatively efficient in contributing to Wiedemann's result. This cause is the difference of electric conductivity in different directions

which may be inferred from Sir W. Thomson's early experiments and from Mr. Tomlinson's recent confirmations and extensions of the conclusions to which he was led regarding the effect of stress on the electric conductivity of metals. It is much to be desired that Mr. Tomlinson should continue his experiments; but in the meantime it seems probable that the electric conductivity in the outer parts of an iron wire twisted within its limits of torsional elasticity is maximum and minimum in the two spirals at 45° to its length, being minimum in that one of them which is of the same name as the twist, that is, the one in the direction of the maximum extension of the substance; and the conductivity is a maximum in the other 45° spiral which is the direction of maximum contraction of the substance. The effect of this æolotropic conductivity, if it exists, must be to cause the electric currents to flow in spirals of opposite spirality to that of the twist and to produce a corresponding amount of longitudinal magnetisation. The effect of this is to develop, at the end by which the current enters, a true south pole when the twist is right-handed, and a true north pole when left-handed, which is opposite to Wiedemann's result. And if the tangential magnetising force exceeds the critical value, the effect of the æolotropic magnetic susceptibility also is opposite to Wiedemann's result. This is a subject of great interest, and requires further investigation.

Photographic Society, May 14.—J. Glaisher, F.R.S., president, in the chair.—Papers were read by Capt. Abney, R.E., F.R.S., on photography at the least refrangible end of the spectrum, and on some photographic phenomena, by W. England, on dry plate processes, and by T. S. Davis, F.C.S., on a tourist's preservative dry plate process.—Capt. Abney in his paper described the means by which he obtained a photograph of the spectrum beyond the B red line by using one of Rutherford's reflection gratings containing 17,280 lines to the inch, which gives a double spectrum outside a central white light, the resulting negative contained 130 perfectly defined lines, many never yet seen by the human eye, the wave length of the lowest lines being about 10,000 tenth metres.

ROME

R. Accademia dei Lincei, Mar. 3.—The following, among other papers, were read:—Geological and palæontological studies on the middle cretaceous of Southern Italy, by M. Sequenza.—On the Italian expedition to Equatorial Africa, by M. Correnti.—On pensile shoots, with measurements of the vertical and horizontal angles, by M. Robert.—Prehistoric Calabrian objects, by M. Ruggeri.—Graphic determination of the forces in reticular woodwork, by M. Favero.—Statistics of the mortality, diseases, and reforms of the Italian army from 1860 to 1875, compared with those of other European armies, by M. Sormano.—On the nummulitic horizon near Castelnuovo dell'Abate, in the province of Siena.

PARIS

Academy of Sciences, June 10.—M. Fizeau in the chair.—The following among other papers were read:—On the cubes or prisms of M. Rohart for the destruction of phylloxera, by M. Chevreul. He finds they contain about thirty per cent. sulphide of carbon. Their efficacy surprises him.—On the large number of joints, mostly perpendicular to each other, which divide the meteoric iron of Santa Catharina (Brazil), by M. Daubrée. In a weight of 23 kilos. were found 1,350 small fragments of iron, each about 17 grammes weight; this would give 25,000 for the 500 kilos. which have come to Europe.—On the source of excito-sudorsal nerve-fibres of the anterior limbs of the cat, by M. Vulpian. They come principally from the spinal nerve, with the spinal roots of the upper thoracic ganglion; but some come directly from the cord by the roots of the nerves forming the brachial plexus.—Experiments proving that the nerve-fibres, whose excitation causes dilatation of the pupil, do not all proceed from the cervical cord of the great sympathetic, by M. Vulpian. Some come directly from the encephalon, mixed probably with fibres of cranial nerves, whose branches enter into connection with the ophthalmic ganglion.—M. Lecoq de Boisbaudran was elected Correspondent for the Section of Chemistry, in room of the late M. Malaguti.—On the geographical distribution of Mexican Graminese, by M. Fournier. He has brought the number up to 638. He divides them into two groups, the one special to Mexico, or partly common to the Andine and northern regions, distinguished by slenderness of leaves and panicles; and the other expanded in the tropical region and noted for larger size. The former inhabit, by preference, mountainous and dry parts; the latter the banks of rivers and moist parts.—On the artificial production of natron or natural carbonate of soda, by the action of carbonate of magnesia on chloride of sodium, by M. Cloez. This is done at ordinary temperature. The author thinks the phenomenon may occur in nature, explaining at once the production of natron and the large quantity of chloride of magnesium found in solution in the water of salt lakes.—On modifications produced in the animal system by various albuminoid substances injected into the vessels, by MM. Bechamp and Baltus. They experimented on dogs both with solutions of natural albumen and with pure albumens of known rotatory power. The latter were not, or were only partly, eliminated.—Influence of the physical state of gallium on its electro-chemical *rôle*, by M. Regnault. He made a small couple (about 489 mm.) of which the two metallic elements were solid and liquid gallium, and were connected by a layer of neutral sulphate of gallium dissolved in water. This caused, in a fine wire galvanometer, constant deflections of more than 40°, in a direction showing that the sheet of liquid had negative tension, while the solid plate had positive. This proves the influence of heat of constitution of a simple metallic body on the energy of its chemical properties.—On starch, by MM. Musculus and Gruber. They give a list of substances produced at expense of starch under the influence of diastase or diluted and boiling sulphuric acid.—Action of fluoride of boron on certain classes of organic compounds, by M. Landolph. Fluoride of boron combines indefinite proportions, equivalent for equivalent, with aldehydes, with acetones, and with carbonyles.—Researches on the peptones, by M. Henninger. These researches seem to indicate that the peptones result from a fixation of water on albuminoid matters, and they thus confirm a hypothesis enunciated by M. Dumas more than thirty years ago that pepsine causes the liquefaction of azotised matters by a phenomenon similar to that of diastase on starch.—Anatomical observations on certain cutaneous excretory glands in the fluviatile tortoises of China, by M. Rathonis. These glands are distinct from those formerly described by Owen and others; their physiological *rôle* is unknown.—Presence and *rôle* of ammoniacal salts in modern seas, and in the saliferous strata of all ages, by M. Dieulafait. All mineral waters, whether sulphurous or not, whether thermal or not, must contain anomalous quantities of ammoniacal salts.—Experimental proof of the incomplete crossing of the nerve-fibres in the chiasma of the optic nerves; longitudinal and median section of the chiasma not followed by blindness, by M. Nicati.

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